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THE TRANSMISSION OF YELLOW FEVER BY MOSQUITOES.

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THE discoveries which have been made during the past twenty-five years with reference to the etiology of infectious diseases constitute the greatest achievement of scientific medicine and afford a substantial basis for the application of intelligent measures of prophylaxis. We now know the specific cause ('germ') of typhoid fever, of pulmonary consumption, of cholera, of diphtheria, of erysipelas, of croupous pneumonia, of the malarial fevers and of various other infectious diseases of man and of the domestic animals, but, up to the present time, all efforts to discover the germ of yellow fever have been without success. The present writer, as a member of the Havana Yellow Fever Commission, in 1879, made the first systematic attempt to solve the unsettled questions relating to yellow fever etiology by modern methods of research. Naturally the first and most important question to engage my attention was that relating to the specific infectious agent, or 'germ,' which there was every reason to believe must be found in the bodies of infected individuals. Was this germ present in the blood, as in the case of relapsing fever; or was it to be found in the organs and tissues which upon post mortem examination give evidence of pathological changes, as in typhoid fever, pneumonia and diphtheria; or was it to be found in the alimentary canal, as in cholera and dysentery. The clinical history of the disease indicated a general blood infection. As my equipment included the best microscopical apparatus made, I had strong hopes that in properly stained preparations of blood taken from the circulation of yellow fever patients my Zeiss 1-18 oil immersion objective would reveal to me the germ I was in search of. But I was doomed to disap-

pointment. Repeated examinations of blood from patients in every stage of the disease failed to demonstrate the presence of micro-organisms of any kind. My subsequent investigations in Havana, Vera Cruz and Rio de Janeiro, made in 1887, 1888 and 1889, were equally unsuccessful. And numerous competent microscopists of various nations have since searched in vain for this elusive germ. Another method of attacking this problem consists in introducing blood from yellow fever patients or recent cadavers into various 'culture media' for the purpose of cultivating any germ that might be present. Extended researches of this kind also gave a negative result, which in my final report I stated as follows:

The specific cause of yellow fever has not yet been demonstrated.

It is demonstrated that micro-organisms, capable of development in the culture-media usually employed by bacteriologists, are only found in the blood and tissues of yellow fever cadavers in exceptional cases, when cultures are made very soon after death.

Since this report was made various investigators have attacked the question of yellow fever etiology, and one of them has made very positive claims to the discovery of the specific germ. I refer to the Italian bacteriologist, Sanarelli. His researches were made in Brazil, and, singularly enough, he found in the blood of the first case examined by him a bacillus. It was present in large numbers, but this case proved to be unique, for neither Sanarelli nor any one else has since found it in such abundance. It has been found in small numbers in the blood and tissues of yellow fever cadavers in a certain number of the cases examined. But carefully conducted researches by competent bacteriologists have failed to demonstrate its presence in a considerable proportion of the cases, and the recent researches of Reed, Carroll and Agramonte, to which I shall shortly refer, demonstrate conclusively that the bacillus of Sanarelli has nothing to do with the etiology of yellow fever.

So far as I am aware, Dr. Carlos Finlay, of Havana, Cuba, was the first to suggest the transmission of yellow fever by mosquitoes. In a communication made to the Academy of Sciences of Havana, in October, 1881, he gave an account of his first attempts to demonstrate the truth of his theory. In a paper contributed to the 'Edinburgh Medical Journal' in 1894 Dr. Finlay gives a summary of his experimental inoculations up to that date as follows:

A summary account of the experiments performed by myself (and some also by my friend, Dr. Delgado), during the last twelve years, will enable the reader to judge for himself. The experiment has consisted in first applying a captive mosquito to a yellow fever patient, allowing it to introduce its lance and to fill itself with blood; next, after the lapse of two or more days, applying the same mosquito to the skin of a person who is considered susceptible to yellow fever; and, finally, observing the effects, not only during the first few weeks, but during periods of several years, so as to appreciate the amount of immunity that should follow.

Between the 30th of June, 1881, and the 2d of December, 1893, eighty-eight persons have been so inoculated. All were white adults, uniting the conditions which justify the assumption that they were susceptible to yellow fever. Only three were women. The chronological distribution of the inoculations was as follows: Seven in 1881, ten in 1883, nine in 1885, three in 1886, twelve in 1887, nine in 1888, seven in 1889, ten in 1890, eight in 1891, three in 1892, and ten in 1893.

The yellow fever patients upon whom the mosquitoes were contaminated were, almost in every instance, well-marked cases of the albuminuric or melano-albuminuric forms, in the second, third, fourth, fifth, or sixth day of the disease. In some of the susceptible subjects, the inoculation was repeated when the source of the contamination appeared uncertain.

Among the eighty-seven who have been under observation, the following results have been recorded:

Within a term of days, varying between five and twenty-five after the inoculation, *one* presented a mild albuminuric attack, and *thirteen only* 'acclimation fevers.'

While Finlay's theory appeared to be plausible and to explain many of the facts relating to the etiology of yellow fever, his experimental inoculations not only failed to give it substantial support, but the negative results, as reported by himself, seemed to be opposed to the view that yellow fever is transmitted by the mosquito. It is true that he reports one case which 'presented a mild albuminuric attack' which we may accept as an attack of yellow fever. But in view of the fact that this case occurred in the city of Havana, where yellow fever is endemic, and of the eighty-six negative results from similar inoculations, the inference seemed justified that in this case the disease was contracted in some other way than as a result of the so-called 'mosquito inoculation.' The thirteen cases in which 'only acclimation fevers' occurred 'within a term of days varying between five and twenty-five after the inoculation' appeared to me to have no value as giving support to Finlay's theory; first, because these 'acclimation fevers' could not be identified as mild cases of yellow fever; second, because the ordinary period of incubation in yellow fever, is less than five days; and, third, because these individuals, having recently arrived in Havana, were liable to attacks of yellow fever, or of 'acclimation fever' as a result of their residence in this city and quite independently of Dr. Finlay's mosquito inoculations. For these reasons Dr. Finlay's experiments failed to convince the medical profession generally of the truth of his theory relating to the transmission of yellow fever, and this important question remained in doubt and a subject of controversy. One party regarded the disease as personally contagious and supposed it to be communicated directly from the sick to the well, as in the case of other contagious diseases, such as smallpox, scarlet fever, etc. Opposed to this theory was the fact that in innumerable instances non-immune persons had been known to care for yellow fever patients as nurses, or physicians, without contracting the

disease; also the fact that the epidemic extension of the disease depends upon external conditions relating to temperature, altitude, rainfall, etc. It was a well-established fact that the disease is arrested by cold weather and does not prevail in northern latitudes or at considerable altitudes, But diseases which are directly transmitted from man to man by personal contact have no such limitations. The alternate theory took account of the above-mentioned facts and assumed that the disease was indirectly transmitted from the sick to the well, as is the case in typhoid fever and cholera, and that its germ was capable of development external to the human body when conditions were favorable. These conditions were believed to be a certain elevation of temperature, the presence of moisture and suitable organic pabulum (filth) for the development of the germ. The two first-mentioned conditions were known to be essential, the third was a subject of controversy.

Yellow fever epidemics do not occur in the winter months in the temperate zone and they do not occur in arid regions. As epidemics have frequently prevailed in sea-coast cities known to be in an insanitary condition, it has been generally assumed that the presence of decomposing organic material is favorable for the development of an epidemic and that, like typhoid fever and cholera, yellow fever is a 'filth disease.' Opposed to this view, however, is the fact that epidemics have frequently occurred in localities (*e. g.,* at military posts) where no local insanitary conditions were to be found. Moreover, there are marked differences in regard to the transmission of the recognized filth diseases —typhoid fever and cholera—and yellow fever. The first-mentioned diseases are largely propagated by means of a contaminated water supply, whereas there is no evidence that yellow fever is ever communicated in this way. Typhoid fever and cholera prevail in all parts of the world and may prevail at any season of the year, although cholera, as a rule, is a disease of the summer months. On the other hand, yellow fever has a very restricted area of prevalence and is essentially a disease of seaboard cities and of warm climates. Evidently neither of the theories referred to accounts for all of the observed facts with reference to the endemic prevalence and epidemic extension of the disease under consideration.

Having for years given much thought to this subject, I became some time since impressed with the view that probably in yellow fever, as in the malarial fevers, there is an 'intermediate host.' I therefore suggested to Dr. Reed, president of the board* appointed upon my recommendation for the study of this disease in the Island of Cuba, that he should give special attention to the possibility of transmission by some

* The members of the board were: Major Walter Reed, Surgeon U. S. A.; Dr. James Carroll, Contract Surgeon U. S. A.; Dr. A. Agramonte, Contract Surgeon U. S. A., and Dr. Jesse W. Lazear, Contract Surgeon U. S. A.

insect, although the experiments of Finlay seemed to show that this insect was not a mosquito of the genus *Culex,* such as he had used in his inoculation experiments. I also urged that efforts should be made to ascertain definitely whether the disease can be communicated from man to man by blood inoculations. Evidently if this is the case the blood must contain the living infectious agent upon which the propagation of the disease depends, notwithstanding the fact that all attempts to demonstrate the presence of such a germ in the blood, by means of the microscope and culture methods, had proved unavailing. I had previously demonstrated by repeated experiments that inoculations of yellow fever blood into lower animals—dogs, rabbits, guinea-pigs—give a negative result, but this negative result might well be because these animals were not susceptible to the disease and could not be accepted as showing that the germ of yellow fever was not present in the blood. A single inoculation experiment on man had been made in my presence in the city of Vera Cruz, in 1887, by Dr. Daniel Ruiz, who was in charge of the civil hospital in that city. But this experiment was inconclusive for the reason that the patient from whom the blood was obtained was in the eighth day of the disease, and it was quite possible that the specific germ might have been present at an earlier period and that after a certain number of days the natural resources of the body are sufficient to effect its destruction, or in some way to cause its disappearance from the circulation.

This was the status of the question of yellow fever etiology when Dr. Reed and his associates commenced their investigations in Cuba during the summer of 1900. In a 'Preliminary Note,' read at the meeting of the American Public Health Association, October 22, 1900, the board gave a report of three cases of yellow fever which they believed to be the direct result of mosquito inoculations. Two of these were members of the board, viz.: Dr. Jesse W. Lazear and Dr. James Carroll, who voluntarily submitted themselves to the experiment. Dr. Carroll suffered a severe attack of the disease and recovered, but Dr. Lazear fell a victim to his enthusiasm in the cause of science and humanity. His death occurred on September 25th, after an illness of six days' duration. About the same time nine other individuals who volunteered for the experiment were bitten by infected mosquitoes—*i. e.,* by mosquitoes which had previously been allowed to fill themselves with blood from yellow fever cases—and in these cases the result was negative. In considering the experimental evidence thus far obtained the attention of the members of the board was attracted by the fact that in the nine inoculations with a negative result "the time elapsing between the biting of the mosquito and the inoculation of the healthy subject varied in seven cases from two to eight days and in the remaining two from ten to thirteen days, whereas in two of the three successful cases the mosquito had been kept

for twelve days or longer." In the third case, that of Dr. Lazear, the facts are stated in the report of the board as follows:

Case 3. Dr. Jesse W. Lazear, Acting Assistant Surgeon U. S. Army, a member of this board, was bitten on August 16, 1900 (Case 3, Table III) by a mosquito *(Culex fasciatus)* which ten days previously had been contaminated by biting a very mild case of yellow fever (fifth day). No appreciable disturbance of health followed this inoculation.

On September 13, 1900 (forenoon), Dr. Lazear, while on a visit to Las Animas Hospital, and while collecting blood from yellow fever patients for study, was bitten by a Culex mosquito (variety undetermined). As Dr. Lazear had been previously bitten by a contaminated insect without after effects, he deliberately allowed this particular mosquito, which had settled on the back of his hand, to remain until it had satisfied its hunger.

On the evening of September 18, 5 days after the bite, Dr. Lazear complained of feeling 'out of sorts,' and had a chill at 8 p. m.

On September 19, 12 o'clock noon, his temperature was 102.4°, pulse 112; his eyes were injected and his face suffused; at 3 p. m. temperature was 103.4°, pulse 104; 6 p. m., temperature 103.8° and pulse 106; albumin appeared in the urine. Jaundice appeared on the third day. The subsequent history of this case was one of progressive and fatal yellow fever, the death of our much-lamented colleague having occurred on the evening of September 25, 1900.

Evidently in this case the evidence is not satisfactory as to the fatal attack being a result of the bite by a mosquito 'while on a visit to Las Animas Hospital,' although Dr. Lazear himself was thoroughly convinced that this was the direct cause of his attack.

The inference drawn by Dr. Reed and his associates, from the experiments thus far made, was that yellow fever may be transmitted by mosquitoes of the genus *Culex,* but that in order to convey the infection to a non-immune individual the insect must be kept for 12 days or longer after it has filled itself with blood from a yellow fever patient in the earlier stages of the disease. In other words, that a certain period of incubation is required in the body of the insect before the germ reaches its salivary glands and consequently before it is able to inoculate an individual with the germs of yellow fever. This inference, based upon experimental data, received support from other observations, which have been repeatedly made, with reference to the introduction and spread of yellow fever in localities favorable to its propagation. When a case is imported to one of our southern seaport cities, from Havana, Vera Cruz or some other endemic focus of the disease, an interval of two weeks or more occurs before secondary cases are developed as a result of such importation. In the light of our present knowledge this is readily understood. A certain number of mosquitoes having filled themselves with blood from this first case after an interval of twelve days or more bite non-immune individuals living in the vicinity, and these individuals after a brief period of incubation fall sick with the disease; being bitten by other mosquitoes they serve to transmit the disease

through the 'intermediate host' to still others. Thus the epidemic extends, at first slowly as from house to house, then more rapidly, as by geometrical progression.

It will be seen that the essential difference between the successful experiments of the board of which Dr. Reed is president and the unsuccessful experiments of Finlay consists of the length of time during which the mosquitoes were kept after filling themselves with blood from a yellow-fever patient. In Finlay's experiments the interval was usually short—from two to five or six days, and it will be noted that in the experiments of Reed and his associates the result was invariably negative when the insect had been kept for less than eight days (7 cases).

Having obtained what they considered satisfactory evidence that yellow fever is transmitted by mosquitoes, Dr. Reed and his associates proceeded to extend their experiments for the purpose of establishing the fact in such a positive manner that the medical profession and the scientific world generally might be convinced of the reliability of the experimental evidence upon which their conclusions were based. These conclusions, which have been fully justified by their subsequent experiments were stated in their 'Preliminary Note' as follows:

1. Bacillus icteroides (Sanarelli) stands in no causative relation to yellow fever, but, when present, should be considered as a secondary invader in this disease.

2. The mosquito serves as the intermediate host for the parasite of yellow fever.

In 'An Additional Note' read at the Pan-American Medical Congress held in Havana, Cuba, February 4-7, 1901, a report is made of the further experiments made up to that date. In order that the absolute scientific value of these experiments may be fully appreciated I shall quote quite freely from this report with reference to the methods adopted for the purpose of excluding all sources of infection other than the mosquito inoculation:

In order to exercise perfect control over the movements of those individuals who were to be subjected to experimentation, and to avoid any other possible source of infection, a location was selected in an open and uncultivated field, about one mile from the town of Quemados, Cuba. Here an experimental sanitary station was established under the complete control of the senior member of this Board. This station was named Camp Lazear, in honor of our late colleague, Dr. Jesse W. Lazear, Acting Assistant Surgeon U. S. A., who died of yellow fever, while courageously investigating the causation of this disease. The site selected was well drained, freely exposed to sunlight and winds, and from every point of view satisfactory for the purposes intended.

The personnel of this camp consisted of two medical officers, Dr. Roger P. Ames, Acting Assistant Surgeon U. S. A., an immune, in immediate charge; Dr. R. P. Cooke, Acting Assistant Surgeon U. S. A., non-immune; one acting hospital steward, an immune; nine privates of the hospital corps, one of whom was immune, and one immune ambulance driver.

For the quartering of this detachment, and of such non-immune individuals as should be received for experimentation, hospital tents, properly floored, were provided. These were placed at a distance of about twenty feet from each other, and were numbered 1 to 7 respectively.

Camp Lazear was established Nov. 20, 1900, and from this date was strictly quarantined, no one being permitted to leave or enter camp except the three immune members of the detachment and the members of the board. Supplies were drawn chiefly from Columbia Barracks, and for this purpose a conveyance under the control of an immune acting hospital steward, and having an immune driver, was used.

A few Spanish immigrants recently arrived at the port of Havana were received at Camp Lazear, from time to time, while these observations were being carried out. A non-immune person, having once left this camp, was not permitted to return to it under any circumstances whatever.

The temperature and pulse of all non-immune residents were carefully recorded three time a day. Under these circumstances any infected individual entering the camp could be promptly detected and removed. As a matter of fact, only two persons, not the subject of experimentation, developed any rise of temperature; one, a Spanish immigrant, with probable commencing pulmonary tuberculosis, who was discharged at the end of three days; and the other, a Spanish immigrant, who developed a temperature of 102.6° F. on the afternoon of his fourth day in camp. He was at once removed with his entire bedding and baggage and placed in the receiving ward at Columbia Barracks. His fever, which was marked by daily intermissions for three days, subsided upon the administration of cathartics and enemata. His attack was considered to be due to intestinal irritation. He was not permitted, however, to return to the camp.

No non-immune resident was subjected to inoculation who had not passed in this camp the full period of incubation of yellow fever, with one exception, to be hereinafter mentioned.

For the purpose of experimentation subjects were selected as follows: From Tent No. 2, 2 non-immunes, and from Tent No. 5, 3 non-immunes. Later, 1 non-immune in Tent No. 6 was also designated for inoculation.

It should be borne in mind that at the time when these inoculations were begun, there were only 12 non-immune residents at Camp Lazear, and that 5 of these were selected for experiment, viz., 2 in Tent No. 2, and 3 in Tent No. 5. Of these we succeeded in infecting 4, viz., 1 in Tent No. 2 and 3 in Tent No. 5, each of whom developed an attack of yellow fever within the period of incubation of this disease. The one negative result, therefore, was in Case 2—Moran—inoculated with a mosquito on the fifteenth day after the insect had bitten a case of yellow fever on the third day. Since this mosquito failed to infect Case 4, three days after it had bitten Moran, it follows that the result could not have been otherwise than negative in the latter case. We now know, as the result of our observations, that in the case of an insect kept at room temperature during the cool weather of November, fifteen or even eighteen days would, in all probability, be too short a time to render it capable of producing the disease.

As bearing upon the source of infection, we invite attention to the period of time during which the subjects had been kept under rigid quarantine, prior to successful inoculation, which was as follows: Case 1, fifteen days; Case 3, nine days; Case 4, nineteen days; Case 5, twenty-one days. We further desire to emphasize the fact that this epidemic of yellow fever, which affected 33.33 per cent. of the non-immune residents of Camp Lazear, did not concern the 7 non-immunes occupying Tents Nos. 1, 4, 6 and 7, *but was strictly limited to those individuals who had been bitten by contaminated mosquitoes.*

Nothing could point more forcibly to the source of this infection than the order of the occurrence of events at this camp. The precision with which the infection of the individual followed the bite of the mosquito left nothing to be desired in order to fulfill the requirements of a scientific experiment.

In summing up their results at the conclusion of this report the following statement is made:

Out of a total of 18 non-immunes whom we have inoculated with contaminated mosquitoes, since we began this line of investigation, 8, or 44.4 per cent., have contracted yellow fever. If we exclude those individuals bitten by mosquitoes that had been kept less than twelve days after contamination, and which were therefore probably incapable of conveying the disease, we have to record eight positive and two negative results—80 per cent.

In a still later report (May, 1901) Dr. Reed says: "We have thus far succeeded in conveying yellow fever to twelve individuals by means of the bites of contaminated mosquitoes."

The non-immune individuals experimented upon were all fully informed as to the nature of the experiment and its probable results and all gave their full consent. Fortunately no one of these brave volunteers in the cause of science and humanity suffered a fatal attack of the disease, although several were very ill and gave great anxiety to the members of the board, who fully appreciated the grave responsibility which rested upon them. That these experiments were justifiable under the circumstances mentioned is, I believe, beyond question. In no other way could the fact established have been demonstrated and the knowledge gained is of inestimable value as a guide to reliable measures of prevention. Already it is being applied in Cuba and without doubt innumerable lives will be saved as a result of these experiments showing the precise method by which yellow fever is contracted by those exposed in an 'infected locality.' Some of these volunteers were enlisted men of the United States Army and some were Spanish immigrants who had recently arrived in Cuba. When taken sick they received the best possible care and after their recovery they had the advantage of being 'immunes' who had nothing further to fear from the disease which has caused the death of thousands and tens of thousands of Spanish soldiers and immigrants who have come to Cuba under the orders of their Government or to seek their fortunes.

The experiments already referred to show in the most conclusive manner that the blood of yellow-fever patients contains the infectious agent, or germ, to which the disease is due, and this has been further demonstrated by direct inoculations from man to man. This experiment was made by Dr. Reed at 'Camp Lazear' upon four individuals, who freely consented to it; and in three of the four a typical attack of yellow fever resulted from the blood injection. The blood was taken from a vein at the bend of the elbow on the first or second day of sickness and was injected subcutaneously into the four non-immune in-

dividuals, the amount being in one positive case 2 c. c., in one 1.5 c. c., and in one 0.5 c. c. In the case attended with a negative result, a Spanish immigrant, a mosquito inoculation also proved to be without effect, and Dr. Reed supposes that this individual 'probably possesses a natural immunity to yellow fever.' Dr. Reed says with reference to these experiments:

It is important to note that in the three cases in which the injection of the blood brought about an attack of yellow fever, careful culture from the same blood, taken immediately after injection, failed to show the presence of Sanarelli's bacillus.

Having demonstrated the fact that yellow fever is propagated by mosquitoes Dr. Reed and his associates have endeavored to ascertain whether it may also be propagated, as has been commonly supposed, by clothing, bedding and other articles which have been in use by those sick with this disease. With reference to the experiments made for the solution of this question I cannot do better than to quote *in extenso* from Dr. Reed's paper read at the Pan-American Medical Congress in Havana. He says:

We believe that the general consensus of opinion both of the medical profession and of the laity is strongly in favor of the conveyance of yellow fever by fomites. The origin of epidemics, devastating in their course, has been frequently attributed to the unpacking of trunks and boxes that contained supposedly infected clothing; and hence the efforts of health authorities, both state and national, are being constantly directed to the thorough disinfection of all clothing and bedding shipped from ports where yellow fever prevails. To such extremes have efforts at disinfection been carried, in order to prevent the importation of this disease into the United States, that, during the epidemic season, all articles of personal apparel and bedding have been subjected to disinfection, sometimes both at the port of departure and at the port of arrival; and this has been done whether the articles have previously been contaminated by contact with yellow-fever patients or not. The mere fact that the individual has resided, even for a day, in a city where yellow fever is present, has been sufficient cause to subject his baggage to rigid disinfection by the sanitary authorities.

To determine, therefore, whether clothing and bedding which have been contaminated by contact with yellow fever patients and their discharges can convey this disease is a matter of the utmost importance. Although the literature contains many references to the failure of such contaminated articles to cause the disease, we have considered it advisable to test, by actual experiment on non-immune human beings, the theory of the conveyance of yellow fever by fomites, since we know of no other way in which this question can ever be finally determined.

For this purpose there was erected at Camp Lazear a small frame house consisting of one room 14 x 20 feet and known as 'Building No. 1,' or the 'Infected Clothing and Bedding Building.' The cubic capacity of this house was 2,800 feet. It was tightly ceiled within with 'tongue-and-grooved' boards, and was well battened on the outside. It faced to the south and was provided with two small windows, each 26 x 34 inches in size. These windows were both placed on the south side of the building, the purpose being to prevent, as much as pos-

sible, any thorough circulation of the air within the house. They were closed by permanent wire screens of .5 mm. mesh. In addition sliding glass sash were provided within and heavy wooden shutters without; the latter intended to prevent the entrance of sunlight into the building, as it was not deemed desirable that the disinfecting qualities of sunlight, direct or diffused, should at any time be exerted on the articles of clothing contained within this room. Entrance was effected through a small vestibule, 3 x 5 feet, also placed on the southern side of the house. This vestibule was protected without by a solid door and was divided in its middle by a wire screen door, swung on spring hinges. The inner entrance was also closed by a second wire screen door. In this way the passage of mosquitoes into this room was effectually excluded. During the day, and until after sunset, the house was kept securely closed, while by means of a suitable heating apparatus the temperature was raised to 92° to 95° F. Precaution was taken at the same time to maintain a sufficient humidity of the atmosphere. The average temperature of this house was thus kept at 76.2° F. for a period of sixty-three days.

Nov. 30, 1900, the building now being ready for occupancy, three large boxes filled with sheets, pillow-slips, blankets, etc., contaminated by contact with cases of yellow fever and their discharges were received and placed therein. The majority of the articles had been taken from the beds of patients sick with yellow fever at Las Animas Hospital, Havana, or at Columbia Barracks. Many of them had been purposely soiled with a liberal quantity of black vomit, urine, and fecal matter. A dirty 'comfortable' and a much-soiled pair of blankets, removed from the bed of a patient sick with yellow fever in the town of Quemados, were contained in one of these boxes. The same day, at 6 p. m., Dr. R. P. Cooke, Acting Assistant-Surgeon U. S. A., and two privates of the hospital corps, all non-immune young Americans, entered this building and deliberately unpacked these boxes, which had been tightly closed and locked for a period of two weeks. They were careful at the same time to give each article a thorough handling and shaking, in order to disseminate through the air of the room the specific agent of yellow fever, if contained in these fomites. These soiled sheets, pillow-cases, and blankets were used in preparing the beds in which the members of the hospital corps slept. Various soiled articles were hung around the room and placed about the bed occupied by Dr. Cooke.

From this date until Dec. 19, 1900, a period of twenty days, this room was occupied each night by these three non-immunes. Each morning the various soiled articles were carefully packed in the aforesaid boxes, and at night again unpacked and distributed about the room. During the day the residents of this house were permitted to occupy a tent pitched in the immediate vicinity, but were kept in strict quarantine.

December 12, a fourth box of clothing and bedding was received from Las Animas Hospital. These articles had been used on the beds of yellow-fever patients, but in addition had been purposely soiled with the bloody stools of a fatal case of this disease. As this box had been packed for a number of days, when opened and unpacked by Dr. Cooke and his assistants, on December 12th, the odor was so offensive as to compel them to retreat from the house. They pluckily returned, however, within a short time and spent the night as usual.

December 19 these three non-immunes were placed in quarantine for five days and then given the liberty of the camp. All had remained in perfect health, notwithstanding their stay of twenty nights amid such unwholesome surroundings.

During the week, December 20-27, the following articles were also placed in this house, viz.: pajamas suits, 1; undershirts, 2; night-shirts, 4; pillow-slips,

4; sheets, 6; blankets, 5; pillows, 2; mattresses, 1. These articles had been removed from the persons and beds of four patients sick with yellow fever and were very much soiled, as any change of clothing or bed-linen during their attacks had been purposely avoided, the object being to obtain articles as thoroughly contaminated as possible.

From Dec. 21, 1900, till Jan. 10, 1901, this building was again occupied by two non-immune young Americans, under the same conditions as the preceding occupants, except that these men slept every night in the very garments worn by yellow fever patients throughout their entire attacks, besides making use exclusively of their much-soiled pillow-slips, sheets, and blankets. At the end of twenty-one nights of such intimate contact with these fomites, they also went into quarantine, from which they were released five days later in perfect health.

From January 11 till January 31, a period of twenty days, 'Building No. 1' continued to be occupied by two other non-immune Americans, who, like those who preceded them, have slept every night in the beds formerly occupied by yellow fever patients and in the night-shirts used by these patients throughout the attack, without change. In addition, during the last fourteen nights of their occupancy of this house they have slept, each night, with their pillows covered with towels that had been thoroughly soiled with the blood drawn from both the general and capillary circulation, on the first day of the disease, in the case of a well-marked attack of yellow fever. Notwithstanding this trying ordeal, these men have continued to remain in perfect health.

The attempt which we have therefore made to infect 'Building No. 1,' and its seven non-immune occupants, during a period of sixty-three days, has proved an absolute failure. We think we cannot do better here than to quote from the classic work of La Roche.* This author says: "In relation to the yellow fever, we find so many instances establishing the fact of the non-transmissibility of the disease through the agency of articles of the kind mentioned, and of merchandise generally, that we cannot but discredit the accounts of a contrary character assigned in medical writings, and still more to those presented on the strength of popular report solely. For if, in a large number of well-authenticated cases, such articles have been handled and used with perfect impunity —and that, too, often under circumstances best calculated to insure the effect in question—we have every reason to conclude that a contrary result will not be obtained in other instances of a similar kind; and that consequently the effect said to have been produced by exposure to those articles, must—unless established beyond the possibility of doubt—be referred to some other agency."

The question here naturally arises: How does a house become infected with yellow fever? This we have attempted to solve by the erection at Camp Lazear of a second house, known as 'Building No. 2,' or the 'Infected Mosquito Building.' This was in all respects similar to 'Building No. 1,' except that the door and windows were placed on opposite sides of the building so as to give through-and-through ventilation. It was divided, also, by a wire-screen partition, extending from floor to ceiling, into two rooms, 12 x 14 feet and 8 x 14 feet respectively. Whereas, all articles admitted to 'Building No. 1' had been soiled by contact with yellow fever patients, all articles admitted to 'Building No. 2' were first carefully disinfected by steam before being placed therein.

On Dec. 21, 1900, at 11.45 a. m., there were set free in the larger room of this building fifteen mosquitoes—*C. fasciatus*—which had previously been contaminated by biting yellow fever patients, as follows: 1, a severe case, on the second day, Nov. 27, 1900, twenty-four days; 3, a well-marked case, on the first

* R. La Roche: Yellow Fever, Vol. II, p. 516, Philadelphia.

day, Dec. 9, 1900, twelve days; 4, a mild case, on the first day, Dec. 13, 1900, eight days; 7, a well-marked case, on the first day, Dec. 16, 1900, five days—total, 15.

Only one of these insects was considered capable of conveying the infection, viz., the mosquito that had bitten a severe case twenty-four days before; while three others—the twelve-day insects—had possibly reached the dangerous stage, as they had been kept at an average temperature of 82° F.

At 12, noon, of the same day, John J. Moran—already referred to as Case 2 in this report—a non-immune American, entered the room where the mosquitoes had been freed, and remained thirty minutes. During this time he was bitten about the face and hands by several insects. At 4.30 p. m., the same day, he again entered and remained twenty minutes, and was again bitten. The following day, at 4.30 p. m., he, for the third time, entered the room, and was again bitten.

Case 7.—On Dec. 25, 1900, at 6 a. m., the fourth day, Moran complained of slight dizziness and frontal headache. At 11 a. m. he went to bed, complaining of increased headache and malaise, with a temperature of 99.6° F., pulse 88; at noon the temperature was 100.4° F., the pulse 98; at 1 p. m., 101.2° F., the pulse 96, and his eyes were much injected and face suffused. He was removed to the yellow fever wards. He was seen on several occasions by the board of experts and the diagnosis of yellow fever confirmed.

The period of incubation in this case, dating from the first visit to 'Building No. 2,' was three days and twenty-three hours. If reckoned from his last visit it was two days and eighteen hours. There was no other possible source for his infection, as he had been strictly quarantined at Camp Lazear for a period of thirty-two days prior to his exposure in the mosquito building.

During each of Moran's visits, two non-immunes remained in this same building, only protected by the wire-screen partition. From Dec. 21, 1900, till Jan. 8, 1901, inclusive—eighteen nights—these non-immunes have slept in this house, only protected by the wire-screen partition. These men have remained in perfect health to the present time.

Thus at Camp Lazear, of 7 non-immunes whom we attempted to infect by means of the bites of contaminated mosquitoes, we have succeeded in conveying the disease to 6, or 85.71 per cent. On the other hand, of 7 non-immunes whom we tried to infect by means of fomites, under particularly favorable circumstances, we did not succeed in a single instance.

It is evident that in view of our present knowledge relating to the mode of transmission of yellow fever, the preventive measures which have heretofore been considered most important, *i. e.*, isolation of the sick, disinfection of clothing and bedding, and municipal sanitation—are either of no avail or of comparatively little value. It is true that yellow fever epidemics have resulted, as a rule, from the introduction to a previously healthy locality of one or more persons suffering from the disease. But we now know that its extension did not depend upon the direct contact of the sick with the non-immune individuals and that isolation of the sick from such contact is unnecessary and without avail. On the other hand complete isolation from the agent which is responsible for the propagation of the disease is all-important. In the absence of a yellow-fever patient from which to draw blood the mosquito is harmless, and in the absence of the mosquito the yellow-fever patient is

harmless—as the experimental evidence now stands. Yellow fever epidemics are terminated by cold weather because then the mosquitoes die or become torpid. The sanitary condition of our southern seaport cities is no better in winter than in summer and if the infection attached to clothing and bedding it is difficult to understand why the first frosts of autumn should arrest the progress of an epidemic. But all this is explained now that the mode of transmission has been demonstrated.

Insanitary local conditions may, however, have a certain influence in the propagation of the disease, for it has been ascertained that the species of mosquito which serves as an intermediate host for the yellow fever germ may breed in cesspools and sewers, as well as in stagnant pools of water. If, therefore, the streets of a city are unpaved and ungraded and there are open spaces where water may accumulate in pools, as well as open cesspools to serve as breeding places for *Culex fasciatus*, that city will present conditions more favorable for the propagation of yellow fever than it would if well paved and drained and sewered.

The question whether yellow fever may be transmitted by any other species of mosquito than *Culex fasciatus* has not been determined. Facts relating to the propagation of the disease indicate that the mosquito which serves as an intermediate host for the yellow-fever germ has a somewhat restricted geographical range and is to be found especially upon the sea-coast and the margins of rivers in the so-called 'yellow fever zone.' While occasional epidemics have occurred upon the southwest coast of the Iberian peninsula, the disease, as an epidemic, is unknown elsewhere in Europe, and there is no evidence that it has ever invaded the great and populous continent of Asia. In Africa it is limited to the west coast. In North America, although it has occasionally prevailed as an epidemic in every one of our seaport cities as far north as Boston, and in the Mississippi Valley as far north as St. Louis, it has never established itself as an endemic disease within the limits of the United States. Vera Cruz, and probably other points on the Gulf coast of Mexico, are, however, at the present time endemic foci of the disease. In South America it has prevailed as an epidemic at all of the seaports on the Gulf and Atlantic Coasts, as far south as Montevideo and Buenos Ayres, and on the Pacific along the coast of Peru.

The region in which the disease has had the greatest and most frequent prevalence is bounded by the shores of the Gulf of Mexico, and includes the West India islands. Within the past few years yellow fever has been carried to the west coast of North American, and has prevailed as an epidemic as far north as the Mexican port of Guaymas, on the Gulf of California.

It must not be supposed that *Culex fasciatus* is only found where

yellow fever prevails. The propagation of the disease depends upon the introduction of an infected individual to a locality where this mosquito is found, at a season of the year when it is active. Owing to the short period of incubation (five days or less), the brief duration of the disease and especially of the period during which the infectious agent (germ) is found in the blood, it is evident that ships sailing from infected ports, upon which cases of yellow fever develop, are not likely to introduce the disease to distant seaports. The continuance of an epidemic on ship-board, as on the land, must depend upon the presence of infected mosquitoes and of non-immune individuals. Under these conditions we can readily understand why the disease should not be carried from the West Indies or from South America to the Mediterranean, to the east coast of Africa or to Asiatic seaport cities. On the other hand, if the disease could be transmitted by infected clothing, bedding, etc., there seems no good reason why it should not have been carried to these distant localities long ago.

The restriction as regards altitude, however, probably depends upon the fact that the mosquito which serves as an intermediate host is a coast species, which does not live in elevated regions. It is a well-established fact that yellow fever has never prevailed in the City of Mexico, although this city has constant and unrestricted intercourse with the infected seaport, Vera Cruz. Persons who have been exposed in Vera Cruz during the epidemic season frequently fall sick after their arrival in the City of Mexico, but they do not communicate the disease to those in attendance upon them or to others in the vicinity. Evidently some factor essential for the propagation of the disease is absent, although we have the sick man, his clothing and bedding and the insanitary local conditions which have been supposed to constitute an essential factor. I am not aware that any observations have been made with reference to the presence or absence of *Culex fasciatus* in high altitudes, but the inference that it is not to be found in such localities as the City of Mexico seems justified by the established facts already referred to.

As pointed out by Hirsch, "the disease stops short at many points in the West Indies where the climate is still in the highest degree tropical." In the Antilles it has rarely appeared at a height of more than 700 feet. In the United States the most elevated locality in which the disease has prevailed as an epidemic is Chattanooga, Tenn., which is 745 feet above sea level.

It will be remembered that the malarial fevers are contracted as a result of inoculation by mosquitoes of the genus *Anopheles,* and that the malarial parasite has been demonstrated not only in the blood of those suffering from malarial infection, but also in the stomach and salivary glands of the mosquito. If the yellow fever parasite resembled that of the malarial fevers it would no doubt have been discovered long

ago. But, as a matter of fact, this parasite, which we now know is present in the blood of those sick with the disease, has thus far eluded all researches. Possibly it is ultra-microscopic. However this may be, it is not the only infectious disease germ which remains to be discovered. There is without doubt a living germ in vaccine lymph and in the virus from smallpox pustules, but it has not been demonstrated by the microscope. The same is true of foot and mouth disease and of infectious pleuro-pneumonia of cattle, although we know that a living element of some kind is present in the infectious material by which these diseases are propagated. In Texas fever, of cattle, which is transmitted by infected ticks, the parasite is very minute, but by proper staining methods and a good microscope it may be detected in the interior of the red blood corpuscles. Drs. Reed and Carroll are at present engaged in a search for the yellow fever germ in the blood and in the bodies of infected mosquitoes. What success may attend their efforts remains to be seen, but at all events the fundamental facts have been demonstrated that this germ is present in the blood and that the disease is transmitted by a certain species of mosquito—*C. fasciatus.*

The proper measures of prophylaxis in view of this demonstration are given in the following circular, which was submitted for my approval by the Chief Surgeon, Department of Cuba, and has recently been published by the Commanding General of that Department, who, until quite recently, was a member of the Medical Corps of the Army:

CIRCULAR, HEADQUARTERS DEPARTMENT OF CUBA,
No. 5. *Havana, April 27, 1901.*

Upon the recommendation of the Chief Surgeon of the Department, the following instructions are published and will be strictly enforced at all military posts in this Department:

The recent experiments made in Havana by the Medical Department of the Army having proved that yellow fever, like malarial fever, is conveyed chiefly, and probably exclusively, by the bite of infected mosquitoes, important changes in the measures used for the prevention and treatment of this disease have become necessary.

1. In order to prevent the breeding of mosquitoes and protect officers and men against their bites, the provisions of General Orders No. 6, Department of Cuba, December 21, 1900, shall be carefully carried out, especially during the summer and fall.

2. So far as yellow fever is concerned, infection of a room or building simply means that it contains infected mosquitoes, that is, mosquitoes which have fed on yellow-fever patients. Disinfection, therefore, means the employment of measures aimed at the destruction of these mosquitoes. The most effective of these measures is fumigation, either with sulphur, formaldehyde or insect powder. The fumes of sulphur are the quickest and most effective insecticide but are otherwise objectionable. Formaldehyde gas is quite effective if the infected rooms are kept closed and sealed for two or three hours. The smoke of insect powder has also been proved very useful; it readily stupefies mosquitoes, which drop to the floor and can then be easily destroyed.

The washing of walls, floors, ceilings and furniture with disinfectants is unnecessary.

3. As it has been demonstrated that yellow fever cannot be conveyed by fomites, such as bedding, clothing, effects and baggage, they need not be subjected to any special disinfection. Care should be taken, however, not to remove them from the infected rooms until after formaldehyde fumigation, so that they may not harbor infected mosquitoes.

Medical officers taking care of yellow-fever patients need not be isolated; they can attend other patients and associate with non-immunes with perfect safety to the garrison. Nurses and attendants taking care of yellow fever patients shall remain isolated, so as to avoid any possible danger of their conveying mosquitoes from patients to non-immunes.

4. The infection of mosquitoes is most likely to occur during the first two or three days of the disease. Ambulant cases, that is, patients not ill enough to take to their beds and remaining unsuspected and unprotected, are probably those most responsible for the spread of the disease. It is therefore essential that all fever cases should be at once isolated and so protected that no mosquitoes can possibly get access to them until the nature of the fever is positively determined.

Each post shall have a 'reception ward' for the admission of all fever cases and an 'isolation ward' for the treatment of cases which prove to be yellow fever. Each ward shall be made mosquito-proof by wire netting over doors and windows, a ceiling of wire netting at a height of seven feet above the floor, and mosquito bars over the beds. There should be no place in it where mosquitoes can seek refuge, not readily accessible to the nurse. Both wards can be in the same building, provided they are separated by a mosquito-tight partition.

5. All persons coming from an infected locality to a post shall be kept under careful observation until the completion of five days from the time of possible infection, either in a special detention camp or in their own quarters; in either case, their temperature should be taken twice a day during this period of observation so that those who develop yellow-fever may be placed under treatment at the very inception of the disease.

6. Malarial fever, like yellow fever, is communicated by mosquito bites and therefore is just as much of an infectious disease and requires the same measures of protection against mosquitoes. On the assumption that mosquitoes remain in the vicinity of their breeding places, or never travel far, the prevalence of malarial fever at a post would indicate want of proper care and diligence on the part of the Surgeon and Commanding Officer in complying with General Orders No. 6, Department of Cuba, 1900.

7. Surgeons are again reminded of the absolute necessity, in all fever cases, to keep, from the very beginning, a complete chart of pulse and temperature, since such a chart is their best guide to a correct diagnosis and the proper treatment.

By Command of Major General Wood:

H. L. SCOTT,
Adjutant General.

CLIMATE AND CARBONIC ACID.*

By BAILEY WILLIS,
U. S. GEOLOGICAL SURVEY.

THE fact that a very extensive and massive ice sheet covered countries of the northern hemisphere which now enjoy a mild climate is generally known and accepted, although it is little more than fifty years since Agassiz (1840-47) made the then novel suggestion to explain the occurrence of glacial deposits where no glaciers remain. It is not so generally known that the great ice age was characterized by the development of several ice sheets in succession, each of them separated from its forerunner by an interval of mild climate during which the ice retreated far toward its source, and but few realize that these intervals of mildness were longer than the time which has elapsed since the latest glaciers withdrew from New England and the northern Central States.

Since the fact of a glacial period was established, several hypotheses have been framed to account for the phenomena of climatic change. As the sun warms the earth, variations in its condition and distance were postulated. As the poles are now regions of glacial accumulation, it was thought that the earth's axis of rotation might have shifted in such a way as to bring the once glaciated regions into polar relations. Or as heights of land are often mantled in snow and ice under latitudes where lowlands are free, glaciation was connected theoretically with a general elevation of continents and mountains. There are facts to sustain most of the speculations thus suggested. Each contains a possible cause. But no one is free from serious question of its sufficiency, while there is little evidence to show that any was definitely related in time to a glacial epoch, except that one which is based on a general elevation of the land.

Professor Chamberlin, of the University of Chicago, long ago advocated a method of investigation known as the method of multiple hypotheses. It calls upon the student to lay aside a natural preference for the theory which seems plausible and to consider as sincerely that which holds out small promise of development. As an earnest student of the causes of the glacial period, he has thus considered every suggestion that might solve that enigma. The astronomical causes, the shifting of the pole, the variations in altitude of the continent, have all been passed in review.

*A review of Chamberlin's 'Working Hypothesis of a Cause of Glacial Epochs.'

In considering the climatic conditions which gave to the coast of southern New England the aspect of Greenland at the present time, thought naturally turned to the antithetic phase when Greenland possessed the climate of Florida. And seemingly linked with these were other climatic variations, such as the great humidity of the Coal Measure period and the great aridity of epochs when salt and gypsum deposits accumulated; while the cause of that redness, which in several continents is characteristic of strata of certain geologic ages, might be traced to world-wide atmospheric conditions. The problem was thus greatly broadened in the scope of related phenomena, and the demands to be met by an adequate hypothesis became correspondingly complex.

The investigation upon which the hypothesis under review rests considers the physics and chemistry of the atmosphere in relation to temperature, the physics and chemistry of the ocean, the interaction of the ocean and the air, and those events of geologic history which as cause or effect may be related to the constitution of the atmosphere. It is not here proposed to review critically the several articles in which Professor Chamberlin and his associates have presented the results of profound researches. Suffice it to endeavor clearly to present an outline of their reasoning and conclusions.

The constitution of the atmosphere has long been known, and in a general way is stated for dry air as 21 parts of oxygen and 79 parts of nitrogen by volume. Argon, a newly discovered component, was formerly measured as nitrogen, and frequently there are impurities, though in small amount. There are 3 to 4 parts of carbonic acid in 10,000, and under natural conditions moisture is present in greater or less proportion. It is with these last, the carbonic acid and moisture, that the student of climatic changes has to deal chiefly.

The functions of carbonic acid and moisture in the atmosphere are threefold. They both absorb radiant heat in an unusual degree. By thus raising the temperature of the air, they both increase its capacity for moisture. And they both are chemically active.

Radiant light and heat penetrate the atmosphere to reach the solid earth, and are in part radiated back through the air into space. As the air is transparent toward light, so is it also toward heat, allowing both forms of energy to pass with moderate absorption. A photographer who compares the exposure of his plate at a considerable altitude with that near sea level roughly measures the relative strength of light at the two places and finds it less beneath the greater depth of atmosphere. The direct heat of the sun's rays is correspondingly less by the sea. The energy which the heated earth radiates back toward space is in part also absorbed by the air, which is thus warmed by the passage of rays to and from the earth.

In this absorption the mass of nitrogen and oxygen has but an

insignificant part. They are nearly perfectly transparent to heat. Carbonic acid and moisture are the effective constituents, which thicken, as it were, the atmospheric blanket, and being warmed in turn keep warm the earth. If they are decreased the blanket becomes thin and the surface grows cool.

Tyndall first suggested that a lessening of the proportion of carbonic acid might suffice to bring on the cold climate of a glacial epoch. He was followed by several investigators who determined more accurately the parts played by carbonic acid and by moisture, Austrian, German and American scientists competing in the study. In 1896, Dr. Arrhenius, a Swedish physicist, reached definite quantitative estimates of the effects. Employing values for the radiant heat of the full moon at different heights above the horizon, measured by Langley, he computed the heat absorbed by the atmosphere. By elaborate calculations he determined that a decrease of carbonic acid in the atmosphere to an amount ranging from 55 to 67 per cent. of the present content would reduce the average temperature 4 or 5 degrees C., which would bring on a glacial epoch, whereas an increase of carbonic acid to an amount two or three times the present content would elevate the average temperature 8 or 9 degrees C., and bring on a mild climate in high latitudes.

The effects of relatively absorbent or transparent atmospheres are not direct and uniform. They vary with the angle of incidence of the sun's rays and, therefore, with latitude, with seasons, and with day and night. They differ with altitude above the earth's surface; they are unlike on land and sea. But in general result the effect of greater absorptive power is to equalize all differences due to geographical and astronomical relations, whereas that of a relatively transparent condition is to accentuate them.

The physicist having thus indicated a possible solution, the further task of framing a working hypothesis was the geologist's. Chamberlin says: "There are hypotheses and working hypotheses. . . . General suggestions of a possible cause do not reach the dignity of working hypotheses until they are given concrete form, are fitted in detail to the specific phenomena and are made the agents of calling into play effective lines of research." In his attempt to frame a working hypothesis of the cause of glacial periods on an atmospheric basis, he has nobly met the requirements of his own definition under the difficult conditions imposed by the phenomena of glaciation. However the resulting working hypothesis may hereafter be modified by further research, its presentation must always stand as an example of the highest scientific effort.

Let us briefly review the requirements of the task. The fundamental postulate of the hypothesis is that variations of the atmospheric

content of carbonic acid have been the direct cause of variations of climate. It is necessary, therefore, to assign agencies adequate to bring about such alternations of poverty and wealth of carbonic acid. The agencies must operate to produce great cycles of climatic change which are recognized through study of the geologic record. Among others, these comprise an ancient event of extensive glaciation in India, Australia and South Africa, closely following the period of mild climate during which the Coal Measure flora flourished. The agencies must further promote subsidiary action by which minor oscillations of climate may be explained, since within the latest, the Pleistocene, period of glaciation, at least five, and probably more, advances of the ice occurred in alternation with intervals of comparative mildness, during which the ice retreated notably. Depletion and enrichment of the atmosphere must furthermore occur within reasonable limits of geologic periods. And cause must be shown why the atmospheric changes promoted glaciation about peculiarly local centers. In searching the sources of carbonic acid, Chamberlin has been led to reconsider the original constitution of the atmosphere, and thus also theories of the origin of the earth, including the nebular hypothesis. Thither this review may not follow him, but it will be of interest to advert to his views as to the conditions affecting biologic evolution, which are also causally related to variations of the carbonic acid contained in the air.

Carbonic acid, or as it is more accurately called, carbon dioxide, CO_2, occurs in many relations and plays many parts in the economy of the world. In some of these activities it enters into permanent combinations and is lost to the atmosphere. In others it passes through cycles of combination and release by which it is temporarily withdrawn from and subsequently returned to the air. If the atmosphere's resources in CO_2 be compared with a bank account, we may suppose that the balance follows one or the other of two familiar cases. In the one example there may have been originally a definite though possibly large deposit, which has not since been added to, but upon which many drafts have been and are being drawn. Under this assumption, however rich the atmosphere once was, it is now by comparison poverty stricken. On the other hand, there is reason to believe that the original capital in CO_2 was not materially greater than it is now, but that losses have been nearly balanced by gains. The first example represents a view held by geologists who believe that the atmosphere was exceedingly dense, moist and charged with carbon dioxide in early ages of the earth's history; the second illustrates the conceptions based on modern advances of biology and geology, and its acceptance is essential to the hypothesis of glaciation here discussed.

The sources from which fresh contributions may be made to the atmosphere are suggested by the occurrence of carbon dioxide among

the gases projected by the sun beyond its gravitative control, in interstellar spaces, in meteorites and in the terrestrial mass. The first three possible sources are too indefinite both in amount and in distribution through the ages to be of any present value to the hypothesis, but the last is important. Crystalline rocks of the superficial crust of the earth are shown by analysis to contain four and one-half times their own volume of gases, of which carbon dioxide, CO_2, and monoxide, CO, form a large percentage. During volcanic eruptions gases and vapors are ejected in indefinitely large volumes. What part of these was once of the atmosphere and is returned to it after an underground journey we do not know, but it is believed that a large part may come from the interior. The escape of these internal gases may also occur in some degree continuously by diffusion, and in influential amounts during episodes of mountain growth, when rock masses are strained and riven and upraised. We shall see presently that in the wasting of a mountain range there is serious consumption of carbon dioxide, which in greater or less degree temporarily affects the gain. The Pleistocene glaciation is attributed to a very notable offset of this character, but the exceptional nature of that event as compared with the relatively frequent episodes of mountain growth indicates that the gain of carbon dioxide has commonly equaled or exceeded the resulting temporary loss.

Among the cycles of combination and release, through which carbon dioxide runs, there are two which are both important, though not equally so. The first and less important is the cycle of organic change, involving plant and animal tissues. When grass grows, carbon dioxide is taken from the air. Grass becomes beef, and beef, through various changes, is resolved into new compounds, yielding back the carbon dioxide to the air. In its brief phases this cycle has no import for the hypothesis, but there are occasions where it is prolonged, as in the accumulations of vegetal substances fossilized as coal. The total amount of carbon dioxide thus abstracted, and now withheld, is very large, and is believed to have been an important factor in promoting at least one instance of glaciation, that which followed closely upon the Coal Measure period.

The more important cycle of combination and release involves the decomposition of rocks by weathering, the solution of certain products and their transportation to the sea, and the reactions through physical, chemical and organic agencies, by which carbon dioxide is either permanently locked up in limestones or is returned to the air.

For the purposes of this statement, the common minerals of rocks may be classified as silicates and carbonates, that is as compounds with silicic acid and compounds with carbonic acid. The former may be typified by the familiar minerals of granite, the latter by limestone.

Granite and all similar crystalline aggregates of silicates disintegrate, and the separate minerals are decomposed chemically by the action of carbon dioxide and moisture. Of the various compounds which result, those of carbon dioxide with lime and magnesia are of most direct interest in this connection, and those with lime may be discussed as representative.

The common combinations of lime and carbon dioxide are two: the carbonate of lime, more specifically called the normal or monocarbonate, and the bicarbonate of lime. The carbonate consists of one ion, or chemical unit, of lime, CaO, combined with one ion of carbon dioxide, CO_2. The bicarbonate consists of one ion of lime combined with two ions of carbon dioxide. The carbonate is but slightly soluble in water, the bicarbonate is easily dissolved. The carbonate is produced in the decomposition of silicates, and great amounts of it which have been derived from this source in past ages are now contained in limestones and other calcareous sedimentary rocks. Whether it exists for a brief time in the weathering of silicates or is, as limestone, exposed to atmospheric waters, the carbonate very readily combines with carbon dioxide, and the bicarbonate is formed in solution. All surface and underground waters contain bicarbonate of lime in greater or less quantity, and enormous volumes are annually conveyed to the sea. It is estimated roughly that the weight of the carbonate of lime thus dissolved and contributed to the sea annually is 2,700,000,000 tons. This is about one-half of the total saline matter dissolved in surface waters annually, and a portion of the remainder consists of carbonates of magnesia, potash and soda.

It has been computed by Professor Chamberlin and his associates that the present supply of carbon dioxide in the atmosphere would be exhausted by the decomposition of silicates in 5,000 to 18,000 years at the present rate of consumption if there were no source of replenishment. It is evident that the amount of carbon dioxide in the atmosphere at any time is the balance between supply and draft, and that it may be more or less as one or the other preponderates. The next step in forming the hypothesis, therefore, is to consider conditions which may produce fluctuations of consumption and contribution.

The consumption of carbon dioxide in weathering of rocks is an effect of erosion, the familiar process which tends to reduce heights of land to a low slope, declining to sea level. This tendency is opposed by those internal forces of the earth's mass which depress sea bottoms and relatively uplift continents and mountain ranges. The persistent attacks of the sun's energy are directed against the earthworks raised by terrestrial forces. It is the fabled fight of the powers of light and air against the powers of the dark underworld; and the former never pause, whereas the latter sleep for ages, and, awaking, exert themselves

mightily for a brief time only. While they rest, mountains waste to hills and hills to plains, and the sea spreads over the margins of sinking continents. When they put forth their strength, mountains grow and continents rise from the waters. Their intermittent activity exerts a potent influence on the constitution of the atmosphere, and is so important to the hypothesis of glaciation that a more definite account of the evidence of its periodic nature is necessary.

Sediments laid beneath the sea are waste of continents. By their characters and volume they may indicate their derivation from far-stretching lands or from near mountains. They often occur spread across the bases of ranges which have been planed away by air and waves. By evidences such as these the physical history of any province may be made out; by comparison of provinces the major events in the history of a continent are ascertained; and by comparison of continental histories the sequence of world stages is studied. For any province the limits of knowledge depend only on the completeness of the record in the local rock series; for each continent the inferences are qualified by difficulties of correlating successive steps from province to province; and world-wide conclusions must necessarily be restricted to the broadest effects.

In the present condition of geologic investigation we know but incompletely the rhythm of continental and marine oscillation, but certain marked epochs are recognized. Seas were extensive, while lands were low and restricted, during epochs known to geologists as the middle Ordovician, the middle Silurian, the early Carboniferous, the late Jurassic and the upper Cretaceous. At these times the consumption of carbon dioxide by rock weathering was comparatively slight, according to hypothesis. On the other hand, lands were wide and seas confined to their basins during the close of the Silurian and beginning of Devonian time, during the Permian and early Triassic periods, and during the Pliocene and Pleistocene. These were epochs of unusual consumption of carbon dioxide.

The climatic effect of depletion of carbon dioxide depends upon the rate at which it is taken from the atmosphere. If it were abstracted slowly a large loss might be compensated by moderate supply, whereas if it were rapidly removed the effect on the atmospheric content might be decided. In this relation the growth of mountains has an important accelerating influence. Although the rate of weathering is conditioned by many factors, elevation is so important that Chamberlin's estimate is probably near the truth. It is that for continental areas the rate of carbonation varies probably more nearly as the square than as the simple ratio of altitudes.

Modern studies of mountain growth have materially changed the views held within a decade by geologists as to the ages of ranges.

Among the rocks of any range there are youngest strata that mark a date earlier than the most remote at which the uplift may have occurred. Impressed with the magnitude and grandeur of mountains, geologists assigned them an antiquity limited only by the age of their component strata, but through the interpretation of landscape forms evidence is now accumulating to show that existing ranges are, as a rule, comparatively young. One interesting conclusion is that we live at a time near the culmination of an epoch of mountain growth, that mountains are now widely distributed and high as compared with those of many preceding periods, and the earth's activity as thus manifested is not materially less now than formerly within known geologic history. The crustal adjustment which produced existing mountain ranges and expanded continents appears to have culminated just before or very early in the Glacial epoch, and the recognition of this fact was the principal basis for the hypothesis that glaciation was related directly to elevation of the land areas. Chamberlin interprets the relation through the influence of rock-weathering upon the carbon dioxide of the air, and attributes the cold period to the resulting thermal transparency of the atmosphere.

The carbon dioxide abstracted from the air by weathering passes into the aqueous circulation of the globe, one-half of it in combination as monocarbonates, the other half superadded to form bicarbonates. A further step in framing the hypothesis is to follow this second part until it shall be returned to the air, which shall thus be reenriched and may promote a period of mild climate.

The ocean is a great reservoir holding carbon dioxide in combination with various bases as bicarbonate. It contains also many other salts. Assuming that all the solids dissolved in sea-water have been derived from the land at a rate of solution equal to that now determined by analyses of river waters, it is possible to make a curious calculation, which shows that the carbonate of lime now in the sea would have accumulated in 60,000 years, whereas the common salt, chloride of sodium, would have required 166,000,000 years. The common salt is not removed from solution, nor is there reason to suppose that there is any special source from which it is concentrated, but which does not supply lime; it may, therefore, be taken as a standard of comparison, which shows that there is much less lime in the sea than we should expect. The deficit is accounted for by the great beds of limestone deposited from the sea at various periods from the long past to the present.

In the ocean, bicarbonate of lime is dissolved in a proportion less than that which the waters can hold in solution, and, according to the principles of the older chemistry, it is under these conditions a fixed combination, which remains dissolved. Should, however, the mono-

carbonate of lime separate from the water as a solid precipitate, the second part of the carbon dioxide would be free to pass from the water into the atmosphere. We are thus led to consider the agencies which have caused the deposition of lime from dilute solution.

Modern views of chemical combinations regard compounds in solution as going through a constant interchange of reactions, by which ions pass continuously from one association to another, as in the grand chain the dancers weave in and out with touch of hands. The dance of the ions, more technically called dissociation, is most active in dilute solutions, and is promoted by higher, retarded by lower temperatures. It has been shown by experiment that bicarbonate of lime may be dissociated by agitating the solution, and there are occurrences of calcareous formations which indicate that the monocarbonate is deposited as a result of such action. Thus it is probable that, through this process, warm seas surrender to the air a notable amount of carbon dioxide, but that the contribution becomes insignificant or ceases when the waters are chilled.

Under favorable conditions the ocean abounds in organisms which secrete normal carbonate of lime as parts of their structures. They swarm in the warm waters of tropical oceanic currents, they exist in multitudes on the warm shallows where the sea spreads over the margins of continental masses with a depth not exceeding 100 fathoms, but they are rare or are replaced by species without hard parts in cold waters. The physiological reactions by which these organisms obtain the normal carbonate from the water are not definitely known. They may take it from bicarbonate in solution, or by reaction on sulphate of lime setting free sulphuric acid, which attacks the bicarbonate. In any case, the effect is to fix one ion of carbon dioxide in the solid normal carbonate, and to free the second ion, which may pass into the air. The enormous volume of organic calcareous deposits now forming, and the massive limestone strata of past ages, largely or wholly of organic origin, attest the importance of the process. Life may be considered the most important of those agencies which restore carbon dioxide to the atmosphere, but it is narrowly conditioned by limitations of habitat and warmth.

Carbon dioxide absorbed in sea-water is yielded to the atmosphere and returned by it under varying conditions of tension of the gas, of barometric pressure, and of temperature. At moderate temperature the sea gives up the gas freely, and would supply a deficiency gradually brought about in the atmosphere. But colder waters hold it faster, and may even take carbon dioxide from an already depleted atmosphere.

Thus the processes of dissociation by chemical and organic agencies and of absorption depend upon temperature, and through this dependence promote the prevailing tendency of climatic changes. If the

change be from warm to cold, cooling waters tighten their grasp on the precious gas that might offset the atmospheric depletion. If they be warmed beneath an air growing rich in carbon dioxide, they become generous of their hoard. The processes are, therefore, auxiliary and intensifying, not initiative.

For the initiative process, which may start the train of effects leading to atmospheric enrichment and a warm epoch, we must refer again to the periodic rest and unrest of the earth's forces.

When, through adjustment of the relations between continental masses and masses beneath the oceans, the internal stresses of the globe have been balanced, the average elevation of lands above sea level is a maximum. The highest rate of consumption of carbon dioxide by weathering may be assumed to follow after a brief but appreciable interval, and from that time forth to diminish. As heights waste and slopes sink low, they become mantled with the residual product of weathering, soil, and efficient contact of carbon dioxide with unaltered rock is limited. When the average height of land is become that of a low plain, carbonation is reduced to a very small part of its maximum activity, and the rate of consumption is slow. At some stage of this change the diminishing rate of depletion may equal and thereafter sink below the rate of supply. Thus the initial condition of a return of milder climate inheres in the transient nature of the cause of a cold epoch. The result might, however, be long delayed, but for the accelerating influence of auxiliary processes, of which, as already stated, life is believed to be the most potent.

It is a recorded fact of geologic history that periods of minimum continental elevation have been periods of extensive marine expansion. These conditions have been associated with remarkable development of marine faunas and with general mildness. Chamberlin was the first to point out a causal relation between these conditions and effects. He entertains the idea that at the climax of an epoch of crustal adjustment, the elevation of continents may be somewhat greater than that required by radial equilibrium. If so, they should in time exhibit a tendency to settle back. In so far as this period of readjustment of balance might suffice for deep general denudation, the subsiding lands would present low plain surfaces to the sea. These conditions would be most favorable for wide migration of the shore from the continental margins far inland, and would result in extensive areas of relatively shallow water. A fauna, which had existed on the narrow slope between the original position of the shore and an oceanic basin, would find its habitat immensely enlarged and favorably conditioned. Responding, it would develop varieties, species and genera until, as sometimes was the case, exuberance ran into unfitness and decadence.

The lowland aspect of continents would be favorable to other

ameliorating influences, as well as to life. Before it could have attained its later phases, the acute thermal transparency of the atmosphere must have given place to moderate absorption, and temperate conditions must have succeeded cold. From waters warmed on widening shallows, carbon dioxide would pass into the air by simple diffusion and by chemical dissociation. But the principal contribution, upon which generally prevailing mildness would attend, would be associated with the active development of lime-secreting life, and this relation is firmly established by observation.

Grand seasons of the eras are thus interpreted by Chamberlin as effects of periodic adjustments of the earth's superficial form to stresses developed within its mass. The causes of these stresses are sought by physicists and geologists in the most profound researches, and for the present, at least, they elude discovery, because the physical and chemical conditions of matter within the earth transcend conditions of observation. But geologic investigation is competent to trace their influence upon aspects of the earth, and not the least valuable result of Chamberlin's thought is the impulse it imparts to studies into the geography and life of the past.

The general hypothesis being thus promisingly developed, some would have been satisfied there to rest the suggestion, and the general reader may be content with the splendidly panoramic view of effects and causes which it embraces. But its author pursues its analysis and application with rigorous questioning, limited only by the bounds of existing knowledge, and where knowledge fails he points out the need of research. We shall touch only upon the principal points of his thorough discussion, the competency of the causes, the oscillations of glaciation, the time limits set by the probable duration of glacial and interglacial epochs and the localization of glaciation in Pleistocene and in Carboniferous times.

As already stated, the Pleistocene glaciation is attributed to depletion of the atmospheric carbon dioxide occasioned by the notable expansion and elevation of lands late in the Pliocene period. It is estimated that in the preceding warm age the land area was 44,000,000 square miles. That of the succeeding expansion at its maximum is computed at 65,000,000 square miles, and the present extent is taken at 54,000,000 square miles. That is to say, the areas are related nearly as $1:1\frac{1}{2}:1\frac{1}{4}$. Elevation, which is more important than extent, was at the time of greatest expansion at least two or three times what it shortly before had been when continents were smaller. In the earlier time of mildness the margins of continents were generally submerged, as the eastern portion of North America now is, affording a roomy habitat for lime-secreting marine life. But with the uplift of continents these seashelves were reduced to narrow zones along the steeps which descend

into oceanic depths. Low, limited lands and wide, warm seas had promoted the flow of carbon dioxide from the waters to the air. Lands elevated and expanded and seas shrunk within their basins reversed the course, and the earth took from the air to give to the waters.

The rate of depletion is capable of reasonable calculation. If the amount of carbon dioxide taken from the atmosphere exceeded by 10 per cent. that supplied to it from all possible sources, 50,000 years would suffice to reduce the content from .18 per cent. to .03 per cent. by weight. And this change would bring on glaciation. There are few students of the earth's history who would be willing to admit that the associated effects of topographic development could have occurred in less, if, indeed, in so short a time, and the causes assigned are thus seen to be fully equal to the task imposed.

The climates of the Glacial period were marked by rhythm recorded in advance and retreat, and re-advance and withdrawal, of the ice front several times repeated. The major changes were as great as that which has intervened between the severest glaciation and the present, and occurred early in the series. The later oscillations declined in both Europe and North America. Such rhythmic rebound from one phase to another and back again is characteristic of phenomena which, though they swing to extremes, themselves set up the action that reverses the movement. The ice sheet itself set the bounds of its possible spread.

Assuming glaciation to be inaugurated and the cold to be intensified by consequent accelerating influences, which need not be detailed here, the depleting process of weathering must be checked by the mantling ice and refrigeration. It is estimated that frost and ice at their maximum effect protected 20 per cent. of the Pleistocene land area. Continued depletion depended on the balance between contribution and abstraction, and it is suggested that 20 per cent. (or whatever may have been the proportion of land area sealed against carbonation) represented the initial preponderance of draft over supply. Whenever the effects of glaciation reduced the consumption of carbon dioxide below the inflow from all sources, the glacial epoch would end and the reaction would begin. Once initiated, it would be accelerated by diffusion and dissociation in the richly stored seas, and by renewed development of life in the warmer waters. The mildness might increase till the great glaciers had vanished, but it could have come to stay only in case the height and area of land had adequately diminished.

Lands remained extensive and elevations great during the Pleistocene period. They were even wider and higher than they are now. As an early ice mantle shrunk it bared rock masses and glacial deposits, which were to a great extent favorably conditioned for chemical attack. The renewed consumption of carbon dioxide in time overbalanced the supply, and glaciation went on again.

What was the period of this climatic pendulum? The answer comes to us vaguely in echoes of Niagara's voices. The cataract began its existence at Lewiston during the retreat of the latest ice sheet. Since that time the gorge has been cut back from Lewiston to the present site of the falls, and it is possible to estimate roughly what time the task has consumed. This episode is one-half or less than one-half of the time elapsed since the beginning of the retreat of the ice from its most advanced position. Thus indefinitely, we may count that something like 40,000 years sped while the climate changed from those Greenland conditions to these which we now enjoy. By similar conservative studies of the effects of deposition and erosion accomplished before the latest glaciation, the duration of the interglacial epoch is found to be several times that of the post-glacial interval; that is, in numbers, 80,000 or 120,000 years or more.

The significance of these figures does not depend upon their precision. They confessedly do but indicate the general magnitude of the times. But they serve to show that those times were more than sufficient for the operation of the causes assigned to produce the observed effects, and thus they sustain the hypothesis. Furthermore, they serve to bring glaciation near to us. In earth history, whose eras are measured by millions of years, events which occurred a hundred thousand years ago are of recent date. We live within the operation of the causes which may hinder or promote glaciation, and, though the present is an age of comparative mildness, we cannot be sure whether this be the spring of a great era or midsummer of an epoch. Are the gnomes of the under-world wearied of mountain building, and have they sunk to rest? Are the shafts of the sun's heat as they traverse the air effectively caught and stored? Does man, consuming fossil carbon in his manifold activities, unconsciously postpone the return of winter?

The cause of a glacial epoch may be found when an adequate cause of cold is linked with the occurrence of glaciation, but the spread of an ice mantle is dependent on snowfall as well as on temperature, and it is through this relation that the peculiar distribution of Pleistocene glaciers may be explained. It will suffice here to state the meteorological conditions which, according to Chamberlin, determined the most striking centers of accumulation, those which were situated in the plains of north-northeastern America.

Studies of polar currents, which free the northern coast of Siberia of ice and crowd it upon the American Arctic Archipelago, combined with the partial data available as to the barometric conditions of the Arctic zone, lead him to the conclusion that in the northern hemisphere the grand movement of the atmosphere from west to east about the globe is oblique to parallels of latitude, and is upon an axis which has

its pole at a point located somewhere north of Hudson Bay. One effect of this oblique rotation is to establish in northern latitudes between 50 and 60 degrees two areas of low barometer and one of high barometer, a disposition which is in strong contrast to the condition in the southern hemisphere, there being a zone of high pressure along the parallel of 35 degrees, south latitude, with decreasing pressure thence toward the Antarctic. These lows and highs differ from those which are familiar as features of daily weather maps, in that they are nearly stationary. The well-known migrant centers converge toward and run into the great fixed centers. The two permanent lows are situated one across the North Atlantic, from Hudson Bay to Scandinavia, the other in the North Pacific, from Japan to southern Alaska. They are centers of inflowing ascending air currents, and are, therefore, characterized by great precipitation. The region of maximum glaciation at the present time lies between them; one conspicuous development occurring in Greenland in the northwest quarter of the Atlantic low, another lying in Alaska in the northeast quarter of the Pacific low. By an analysis of the winds, it is shown that both Greenland and Alaska lie to leeward of the prevailing currents where they pass ashore. They are not necessarily the provinces of maximum precipitation, rain and snow both considered, but they are areas of copious snowfall, with low annual mean temperatures.

The northern high lies nearly midway between the two lows over Siberia. In contrast to them, it is a center of descending outward-flowing currents, marked by slight precipitation, and it is not now, nor was it in Pleistocene time, a scene of glacial development.

The centers of Pleistocene glaciation were so arranged with reference to the glacial regions of to-day that they would be determined by the oblique circulation and distribution of areas of low pressure, if existing conditions were intensified. An adequate occasion of intensification is found in the thermal transparency of the atmosphere, resulting from depletion of carbon dioxide, and thus the localization of Pleistocene ice sheets is explained in a manner consistent with the major hypothesis of the cause of glaciation.

Chamberlin's hypothesis is framed on an atmospheric basis, but the efficiency of the agencies which it postulates depends upon geographic conditions, upon distribution of land and sea and average heights of continents. The geography of the earth in the closing epochs of the Paleozoic era is known only in its broadest outlines, and they are but vaguely traced. With such imperfect data it is impracticable to explain satisfactorily the extraordinary phenomena of glaciation at that date in intimate association with the development of coal beds and extending within the tropics. Nevertheless, to carry out his purpose of developing a working hypothesis, the author feels obliged to arrange

the known facts and to discuss them along the lines so successfully followed in reference to Pleistocene glaciation. An important difference in the argument as applied to the two cases lies in the cause assigned for depletion of the atmospheric carbon dioxide. The postulate of an appropriate time relation between Paleozoic land movements and the epoch of glaciation being conservatively assigned a minor position, the storage of carbon as coal is given major rank in accordance with known relations. This process is not attended by the accelerating and reacting influences, which are due to the equivalent of carbon dioxide contained in bicarbonates, and glaciation would, therefore, result only after depletion had continued longer. In this suggestion is found a possible reason for the wide extension of cool climate and the occurrence of glaciation in remarkably low latitudes.

The broad scope of philosophic thought upon which this working hypothesis rests is indicated by the titles of articles which have flowed from Chamberlin's pen in the last four years.* Fortifying his own general researches where needed by those of specialists, he, with reason, challenges fundamental and generally accepted views. He gives the geologist and biologist new clues with which to thread the labyrinth of knowledge, and develops important relations between dynamical geology, stratigraphy, climate and evolution.

* A Group of Hypotheses bearing on Climatic Changes, Jour. Geol., Vol. V., No. 7, 1897. The Ulterior Basis of Time Divisions and the Classification of Geologic History, ibid., Vol. VI., No. 5, 1898. A Systematic Source of Evolution of Provincial Faunas, ibid., No. 6, 1898. The Influence of Great Epochs of Limestone Formation upon the Constitution of the Atmosphere, ibid. Lord Kelvin's Address on the Age of the Earth as an Abode Fitted for Life, Science, N. S., Vol. IX., No. 235, pp. 889-901, June 30, 1899, and Vol. X., No. 236, pp. 11-18, July 7, 1899.

THE PEOPLING OF THE PHILIPPINES.*

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SINCE the days when the first European navigators entered the South Sea, the dispute over the source and ethnic affiliations of the inhabitants of that extended and scattered island world has been unsettled. The most superficial glance points out a contrariety in external appearances, which leaves little doubt that here peoples of entirely different blood live near and among one another. And this is so apparent that the pathfinder in this region, Magellan, gave expression to the contrariety in his names for tribes and islands. Since dark complexion was observed on individuals in certain tribes and in defined areas, and light complexion on others, here abundantly, there quite exceptional, writers applied Old World names to the new phenomena without further thought. The Philippines set the decisive example in this. Fernando Magellan first discovered the islands of this great archipelago in 1521, March 16. After his death the Spaniards completed the circle of his discoveries. At this time the name of Negros was fixed,† which even now is called Islad de los Pintados. For years the Spaniards called the entire archipelago Islas de Poniente; gradually, after the expedition of Don Fray Garcia Jofre de Loaisa (1526), the new title of the Philippines prevailed, through Salazar. The people were divided into two groups, the Little Negroes or Negritos and the Indios. It is quite conceivable that involuntarily the opinion prevailed that the Negritos had close relationship with the African blacks, and the Indios‡ with the lighter-complexioned inhabitants of India, or at least of Indonesia.

However, it must be said here that the theory of a truly African origin of the Negritos has been advanced but seldom, and then in a very hesitating manner. The idea that with the present configuration

* Sitzungsberichte der Königlich Preussischen Akademie der Wissenschaften zu Berlin. Berlin, 1897, January-June, 279-289. Translated with notes by Professor O. T. Mason for the annual report of the Smithsonian Institution, and printed from an advance copy supplied by Professor Samuel P. Langley, secretary of the Institution.

† NOTE.—The island of Negros received its name because it was peopled chiefly by a dark, woolly-haired race, while in other islands these were confined to the interior. Cf. A. B. Meyer, Negritos, 1899, p. 16.—TRANSLATOR.

‡ This word, except in an historical sense, should never be used for non-Negrito Filipinos.

of the eastern island world, especially with their great distances apart, a variety of mankind that had never manifested any aptitude for maritime enterprises should have spread themselves over this vast ocean area, in order to settle down on this island and on that, is so unreasonable that it has found scarcely a defender worth naming. More and more the blacks are coming to be considered the original peoples, the 'Indios' to be the intruders. For this there is a quite reasonable ground, in that on many islands the blacks dwell in the interior, difficult of access, especially in the dense and unwholesome mountain forests, while the lighter complexioned tribes have settled the coasts. To this are added linguistic proofs, which place the lighter races, of homogeneous speech, in linguistic relations with the higher races, especially the Malays. Dogmatically it has been said that originally these islands had been occupied entirely by the primitive black population, but afterwards, through intrusions from the sea, these blacks were gradually pressed away from the coast and shoved back into the interior.

The problem, though it appears simple enough, has become complicated more and more through the progress of discovery, especially since Cook enlarged our knowledge of the oriental island world. A new and still more pregnant contrast then thrust itself to the front in the fact that the blacks and the lighter-colored peoples are each separated into widely differing groups. While the former hold especially the immense, almost continental, regions of Australia (New Holland) and New Guinea, and also the larger archipelagos, such as New Hebrides, Solomon Islands, Fiji (Viti) Archipelago—that is, the western areas—the north and east, Micronesia and Polynesia, were occupied by lighter-colored peoples. So the first divison into Melanesia and Polynesia has in latest times come to be of value and the dogma once fixed has remained. For the Polynesians are by many allied to the Malays, while the blacks are put together as a special ethnological race.

For practical ethnology this division may suffice. But the scientific man will seek also for the blacks a genetic explanation. The answer has been furnished by one of the greatest ethnologists, Theodor Waitz,* who, after he had exposed the insufficiency of the accepted formulas, came to the conclusion that the differentiation of the blacks from the lighter peoples might be an error. He denied that there had been a primitive black race in Micronesia and Polynesia; in his opinion we have here to do with a single race. The color of the Polynesians may be out and out from natural causes different; indeed, 'their entire physical appearance indicates the greatest variability.' Herein the

* Anthropologie der Naturvölker, Vol. V; The South Sea Islanders, Part II; The Micronesians and Northwestern Polynesians. Leipzig, 1870, pp. 33-36. Finsch, Verh. d. Berliner Anthrop. Ges., 1882, p. 164.

whole question of the domain of variation is sprung with imperfect satisfaction on the part of those travelers who give their attention more to transitions than to types. Among these are not a few who have returned from the South Sea with the conviction that all criteria for the diagnosis of men and of races are valueless.

Analytical anthropology has led to other and often unexpected results. It has proved that just that portion of South Sea population which can apparently lay the strongest claim to be considered a homogeneous race must be separated into a collection of subvarieties. Nothing appears more likely than that the Negritos of the Philippines are the nearest relatives to the Melanesians, the Australians, the Papuans; and yet it has been proved that all these are separated one from another by well-marked characters. Whether these characters place the peoples under the head of varieties, or whether, indeed, the black tribes of the South Sea, spite of all differences, are to be traced back to one single primitive stock, that is a question of prehistory for whose answer the material is lacking.* Were it possible to furnish the proof that the black populations of the South Sea were already settled in their present homes when land bridges existed between their territory and Africa, or when the much-sought Lemuria still existed, it would not be worth the trouble to hunt for the missing material. In our present knowledge we can not fill the gaps, so we must yet hold the blacks of the Orient to be separate races.†

The hair furnished the strongest character for diagnosis, in which, not alone that of the head is under consideration; the hair, therefore, occupies the foreground of interest. Its color is of the least importance, since all peoples of the South Sea have black hair. It is more the structure and appearance which furnish the observer convenient starting points for the primary classification. Generally a twofold division satisfies. The blacks, it is said, have crisped hair, the Polynesians and light-colored peoples have smooth hair. But this declaration is erroneous in its generality. It is in no way easy to declare absolutely what hair is to be called crisp, and it is still more difficult to define in what respects the so-called crisp varieties differ one from another. For a long time the Australian hair was denominated crisp, until it was evident that it could be classed neither with that of the Africans nor with that of the Philippine blacks. Semper, one of the first travelers to furnish a somewhat complete description of the physical characters of the Negritos, describes it as an "extremely thick, brown-black,

* NOTE.—The reader must consult, on the identity of Negritos with Papuans, A. B. Meyer in Zeitschrift für Ethnologie, Verhandl., Berlin, 1875, p. 47, and the Distribution of the Negritos, Dresden, 1899, pp. 76-87.—TR.

† On Lemuria cf. A. R. Wallace, Geog. Distrib. of Animals, 1876, I, p. 272, and Island Life, 1880, p. 394.—TR.

lack-luster, and crisp-woolly crown of hair."* Among these peculiarities the lack-luster is unimportant, since it is due to want of care and uncleanliness. On the contrary, the other data furnish true characters of the hair, and among them the crisp-woolly peculiarity is most valuable.

On the terms 'wool' and 'woolly' severe controversies, which have not yet closed, have taken place among ethnologists during the last ten years. Also the lack of care, especially the absence of the comb, has here acted as a disturbing cause in the decision. But there is yet a set of peoples, which were formerly included, that are now being gradually disassociated, especially the Australians and the Veddahs, whose hair, by means of special care, appears quite wavy if not entirely sleek and smooth. Generally it is frowzy and matted, so that its natural form is difficult to recognize. To it is wanting the chief peculiarity, which obtrudes itself in the African blacks so characteristically that the compact spiral form which it assumes from its root, the so-called 'pepper-corn,' is selected as the preferable mark of the race. The peculiar nappy head has its origin in the spiral *'rollchen.'* As to the Asiatic blacks this has been for a long time known among the Andamanese; it has lately been noticed upon the Sakai of Malacca, and it is to be found also among the Negritos of the Philippines, as can be shown by specimens. Therefore, if we seek ethnic relationships for the Negritos of the Philippines, or as they are named, the Aetas (Etas, Itas), such connections obtrude themselves with the stocks named, and the more strongly since they all have brachycephalic, relatively small (nannocephalic) heads and through their small size attach themselves to the peculiar dwarf tribes.

I might here comment on the singular facts that the Andaman Islands are situated near the Nicobars in the Indian Ocean, but that the populations on both sides of them are entirely different. In my own detailed descriptions which treat of the skulls and the hair specially,† it is affirmed that the typical skull shape of the Nicobarese is dolichocephalic and that "their hair stands between the straight hair of the Mongoloid and the sleek, though slightly curved or wavy, hair of the Malayan and Indian peoples"; their skin color is relatively dark, but only so much so as is peculiar to the tribes of India. With the little blacks of the Andamans there is not the slightest agreement. In this we have one of the best evidences against the theory of Waitz-Gerland that the differences in physical appearance are to be attributed to variation merely. I will, however, so as not to be misunderstood, expressly emphasize that I am not willing to declare

* Die Philippinen und ihre Bewohner, Würzburg, 1869, p. 49.

† Verhandl. der Berliner Anthrop. Gesellschaft, 1885, pp. 104, 109.

that the two peoples have been at all times so constituted; I am now speaking of actual conditions.

In the same sense I wish also my remarks concerning the Negritos to be taken. Not one fact is in evidence from which we may conclude that a single neighboring people known to us has been Negritized. We are therefore justified when we see in the Negritos a truly primitive people. As they are now, they were more than three hundred and fifty years ago when the first European navigators visited these islands. About older relationships nothing is known. All the graves from which the bones of Negritos now in possession were taken belong to recent times, and also the oldest descriptions which have been received, so far as phylogeny is concerned, must be characterized as modern. * * *

Whoever would picture the present ethnic affiliations of the light-colored peoples of the Philippines will soon land in confusion on account of the great number of tribes. One of the ablest observers, Ferd. Blumentritt,* mentions, besides the Negritos, the Chinese and the whites, not less than 51 such tribes. He classifies them in one group as Malays, according to the plan now customary. This division rests primarily on a linguistic foundation. But when it is noted that the identity of language among all the tribes is not established and among many not at all proved, it is sufficiently shown that speech is a character of little constancy, and that a language may be imposed upon a people to the annihilation of their own by those who belong to a different linguistic stock. The Malay Sea is filled with islands on which tarry the remnants of peoples not Malay.

For a long time, especially since the Dutch occupation, these old populations have received the special name of Alfuros.† But this ambiguous term has been used in such an arbitrary and promiscuous fashion that latterly it has been well-nigh banished from ethnological literature. It is not long ago that the Negritos were so called. But if the black peoples are eliminated, there remains on many islands at least an element to be differentiated from the Malay, chiefly through the darker skin color, greater orthocephaly, and more wavy, quite crimped hair. I have, for the different islands, furnished proof, and will here only refer to the assertion that "a broad belt of wavy and curly hair has pressed itself in between the Papuan and the Malay, a belt which in the north seems to terminate with the Veddah, in the south with the Australian." One can not read the accounts of travelers without the increasing conviction of the existence of several different,

* Versuch einer Ethnographie der Philippinen, Petermann's Mittheilungen, Gotha, 1882, No. 67.

† A. Lesson. Les Polynesians, Paris, 1880, Vol. I, pp. 267, 283. [On this objectionable word see A. B. Meyer, The Distribution of the Negritos, Dresden, 1899, Stengel, p. 7.—Tr.]

if not perhaps related, varieties of peoples thrust on the same island.

From this results the natural and entirely unprejudiced conclusion, which has repeatedly been stated, that either a primitive people by later intrusions has been pressed back into the interior or that in course of time several immigrations have followed one another. At the same time it is not unreasonable to think that both processes went on at the same time, and indeed this conception is strongly brought forward.* So Blumentritt assumes that there is there a primitive black people and that three separate Malay invasions have taken place. The oldest, whose branches have many traits in accord with the Dayaks of Borneo, especially the practice of head-hunting; a second, which also took place before the arrival of the Spaniards, to which the Tagals, Visayas, Vicols, Ilocanes, and other tribes belong; the third, Islamitic, which emigrated from Borneo and might have been interrupted by the arrival of the Spaniards, and with which a contemporaneous immigration from the Moluccas went on. It must be said, however, that Blumentritt admits two periods for the first invasion. In the earliest he places the immigration of the Igorrotes, Apayos, Zambales—in short, all the tribes that dwelt in the interior of the country later and were pressed away from the coast, therefore, actually, the mountain tribes. To the second half he assigns the Tinguianes, Catalanganes, and Irayas, who are not head-hunters, but Semper says they appear to have a mixture of Chinese and Japanese blood.†

Against this scheme many things may be said in detail, especially that, according to the apparently well-grounded assertions of Muller-Beeck, the going of the Chinese to the Philippines was developed about the end of the fourteenth century, and chiefly after the Spaniards had gotten a foothold and were using the Mexican silver in trade. At any rate, the apprehension of Semper, which rests on somewhat superficial physiognomic ground, is not confirmed by searching investigations. So the head-hunting of the mountain tribes, so far as it hints at relations with Borneo, gives no sure chronological result, since it might have been contemporaneous in them and could have come here through invasion from other islands.

The chief inquiry is this: Whether there took place other and older invasions. For this we are not only to draw upon the present tribes,

* R. Virchow, Alfuren-Schädel von Ceram und von den Molucken. Verhandl. Berl. Anthrop. Gesellschaft, 1882, p. 78; 1889, pp. 159, 170. [Whether this be a new type or mixture cf. J. G. F. Riedel, Kroesharige Rassen tuesschen Selebes en Papua, 1886.—TRANSLATOR.]

† NOTE.—The dates for these several migrations are given as follows: First migration, 200 B. C.; second migration 100-500, A. D., bringing the alphabet; third migration, fourteenth and fifteenth centuries, Islamitic. But these dates represent only opinions up to date, from which more thorough inquiry must set out.—TRANSLATOR.

but if possible upon the remains of earlier and perhaps now extinct tribes. This possibility has been brought nearer for the Philippines through certain cave deposits. We have to thank, for the first information, the traveler Jagor, whose exceptional talent as collector has placed us in the possession of rich material, especially crania. To his excellent report of his journey I have already dedicated a special chapter, in which I have presented and partially illustrated not only the cave crania, but also a series of other skulls. An extended conference upon them has been held in the Anthropological Society.*

The old Spanish chroniclers describe accurately the mortuary customs which were in vogue in their time. The dead were laid in coffins made from excavated tree trunks and covered with a well-fitting lid. They were then deposited on some elevated place, or mountain, or river bank, or seashore. Caves in the mountains were also utilized for this purpose. Jagor describes such caves on the island of Samar, west of Luzon, whose contents have recently been annihilated.†

The few crania from there which have been intrusted to me bear the marks of recent pedigree, as also do the additional objects. Unfortunately, Dr. Jagor did not himself visit these interesting caves, but he has brought crania thence which are of the highest interest, and which I must now mention.

The cave in question lies near Lanang,‡ on the east coast of Samar, on the bank of a river, it is said. It is, as the traveler reports, celebrated in the locality "on account of its depressed gigantic crania, without sutures." The singular statement is made clear by means of a well-preserved example, which I lay before you. The entire cranium, including the face, is covered with a thick layer of sinter, which gives it the appearance of belonging to the class of skulls with Leontiasis ossea. It is, in fact, of good size, but through the incrustation it is increased to gigantic proportions. It is true, likewise, that it has a much flattened, broad and compressed form. The cleaning of another skull has shown that artificial deformation has taken place, which obviously was completed before the incrustation was laid on by the mineral water of the cave. I will here add that on the testimony of travelers no Negritos were on Samar. The island lies in the neighborhood of the Visayas. Although no description of the position of the skull is at hand and of the skeleton to which it apparently belonged, it

* NOTE.—In the matter of evidence for high antiquity and separate race furnished by incrusted cave crania, Prof. William H. Holmes's paper on the Calaveras skull (printed in this volume), should be studied, in which serious doubts are thrown upon the value of such relics as witnesses.—TRANSLATOR.

† F. Jagor, Grabstätten zu Nipa-Nipa. Zeitschrift für Ethnologie, 1869, I, p. 80.

‡ Die Philippinen und ihre Bewohner. Verh. der Berliner Anthrop. Gesellsch., 1870, session of 25th of January.

must be assumed that the dead man was not laid away in a coffin, but placed on the ground; that, in fact, he belonged to an earlier 'period.' How long ago that was can not be known, unfortunately, since no data are at hand; however, the bones are in a nearly fossilized condition, which allows the conclusion that they were deposited long ago.

The deformation itself furnishes no clue to a chronological conclusion. In Thévenot* is found the statement that, according to the account of a priest, probably in the 16th century, the custom prevails in some of the islands to press the heads of new-born babes between two boards, also to flatten the forehead, 'since they believed that this form was a special mark of beauty.' A similar deformation, with more pronounced flattening and backward pressure of the forehead, is shown on the crania which Jagor produced from a cave at Caramuan in Luzon. There are modes of flattening which remind one of Peru. When they came into our hands it was indeed an immense surprise, since no knowledge of such deformation in the South Sea was at hand. First our information led to more thorough investigations; so we are aware of several examples of it from Indonesia and, indeed, from the South Sea (Mallicolo). However, this deformation furnishes no clue to the antiquity of the graves.†

I have sawed one of these skulls in two along the sagittal suture. The specimen gives a good idea of the amount of compression and of the violence which this skull endured when quite young. The cranial cavity is inclined backward and lengthened, and curves out above, while the occiput is pressed downward and the region of the front fontanelle is correspondingly lacking. Likewise, a considerable thickness of the bone is to be noted, especially of the vertex. The upper jaw is slightly prognathous and the roof of the mouth unusually arched.

For the purpose of the present study, it is unnecessary to go further into particulars. It might be mentioned that all Lanang skulls are characterized by their size and the firmness of bone, so that they depart widely from the characteristics of the other Philippine examples known to me. Similar skulls have been received only from caves, which exist in one of the little rocky islands east from Luzon. They suggest most Kanaka crania from Hawaii, and Maiori crania from Chatham islands, and they raise the question whether they do not belong to a migration period long before the time of the Malays. I

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* Rélations des diverses voyages curieux. Paris, 1591 (1663).

[† Chinese and Korean pottery are said to have been found with the deformed crania. Similar deformations exist in the Celebes, New Britain, etc. Head-shaping has been universal, cf. A. B. Meyer, Uber kunstliche deformirte Schädel von Borneo und Mindanao and über die Verbreitung der Sitte der kunstlichen Schädeldeformirung, 1881, 36 pp., 4°.—TRANSLATOR.]

have, on various occasions, mentioned this probable pre-Malayan, or at least proto-Malayan, population which stands in nearest relation to the settling of Polynesia. Here I will merely mention that the Polynesian sagas bring the progenitor from the west, and that the passage between Halmahera (Gilolo) and the Philippines is pointed out as the course of invasion.

At any rate, it is quite probable that the skulls from Lanang, Cragaray and other Philippine islands are the remains of a very old, if not autochthonous, prehistoric layer of population. The present mountain tribes have furnished no close analogies. As to the Igorrotes, which Blumentritt attributes to the first invasion, I refer to my description* given on the ground of chronological investigations; according to the account given by Hans Meyer† the disposal of the dead in log coffins and in caves still goes on. Of the skulls themselves, none were brachycephalous; on the contrary, they exhibit platyrrhine and in part decidedly pithecoid noses. On the whole, I came to the conclusion, as did earlier Quatrefages and Hamy, that 'they stand next in comparison with the Dayaks of Borneo,' but I hold yet the impression that they belong to a very old, probably pre-Malay, immigration.‡

* Schädel der Igorroten. Verhl. der Berliner Anthrop. Gesellsch., 1883, pp. 390, 399. [On the Igorrotes see A. B. Meyer, Negritos, 1899, p. 12, note 2. —TRANSLATOR.]

† Die Igorroten von Luzon, p. 386.

‡ With this study of crania should be read Dr. A. B. Meyer, on craniological data and their value, in The Distribution of the Negritos, Dresden, 1899, in which he says: "The form of the skull in general is variable and can not be regarded as a permanent character in the development of the races." The reader must not neglect Dr. Meyer's publications, since in them he has the results of careful studies on the spot: Volume VIII, of the folio publications of the Dresden Royal Ethnographic Museum, 1890, on the tribes of Northern Luzon; Volume IX, of the same, on the Negritos, 1893; Album of Philippine Types, 1885, 32 plates, 4°; ditto, 1891, 50 plates; and The Distribution of the Negritos in the Philippine Islands and Elsewhere, Dresden. The last three are published by Stengel & Co., Dresden. The little book on distribution is in English, and contains, in addition to most useful information, a list of Blumentritt's publications.—TRANSLATOR.

A STUDY OF BRITISH GENIUS.

By HAVELOCK ELLIS.

VIII.—PATHOLOGY.

IN a large proportion of cases no reference is made by the national biographers to the diseases from which their subjects suffered, nor to the general state of health. This, however, we could scarcely expect to find, except in those cases in which the state of health had an obvious influence on the life and work of the eminent person. In most of these exceptional cases it is probable that the biographers have duly called attention to the facts, and though the information thus attained is not always precise—in part owing to the imperfection of the knowledge transmitted, in part to the medical ignorance of the biographers, and in part to the deliberate vagueness of their reference to 'a painful malady,' etc.—it enables us to reach some very instructive conclusions concerning the pathological conditions to which men of genius are most liable.

Putting aside the cases of delicate health in childhood, with which I have already dealt in a previous section, the national biographers state the cause of death, or mention serious diseased conditions during life, in 322 cases.

It is natural to find that certain diseased conditions which are very common among the ordinary population are also very common among men of preeminent intellectual ability. Thus, a lesion of the vessels in the brain (the condition commonly described as paralysis, apoplexy, effusion on the brain, etc.) is a very common cause of death among the general population, and we also find that it is mentioned thirty-five times by the national biographers. Consumption, also, so prevalent among the general population, occurred in at least thirty cases. While many of the consumptive men of genius lived to past middle age, or even reached a fairly advanced age, the disease is responsible for the early death of most of the more eminent of those men of genius who died young—of Keats in poetry, of Bonington and Girtin in art, of Purcell (probably) in music. Some appear to have struggled with consumptive tendencies during a fairly long life; these have usually been men of letters, and have sometimes shown a feverish literary activity, their intellectual output being perhaps more remarkable for quantity than quality. But Sterne in literature, and Black, Priestley, Clifford and other eminent men of science are to be found among the consumptives. It is evident that the disease by no means stands in the way of all but the very highest intellectual attainments, even if it is not indeed actually favorable to mental activity.

Other forms of lung diseases are only mentioned fifteen times. The striking point here is the remarkable frequency of asthma in so small a group. It occurs nine times. It is fairly evident that in nearly all these cases we are concerned with true spasmodic asthma, a malady of the nervous system, and apt to arise, often in early life, on the basis of a somewhat neurotic organism.

Another malady to which we may judge that men of intellectual eminence are specially liable, since it is so often referred to, is angina pectoris. Heart disease—doubtless because its exact diagnosis is of comparatively recent date—is only referred to eighteen times, but in as many as eight or nine of these cases the disease is either distinctly stated to be, or may reasonably be inferred to be, angina pectoris. None of these cases are purely literary men, but four of them are artists.

There is, however, a pathological condition which occurs so often, in such extreme forms, and in men of such preeminent intellectual ability, that it is impossible not to regard it as having a real association with such ability. I refer to gout. This is by no means a common disease, at all events at the present day. In ordinary English medical practice at the present day, it may safely be said that cases of gout seldom form more than one per cent. of the chronic disorders met with. Yet gout is of all diseases that most commonly mentioned by the national biographers; it is noted as occurring in thirty-eight cases, often in very severe forms. We have, indeed, to bear in mind that gout has been recognized for a very long time, and that it is moreover a disease of good reputation. Yet, even if we assume that it has been noted in every case in which it occurs among our 902 eminent persons (an altogether absurd assumption to make), we should still have to recognize that it occurs in over four per cent. Moreover, the eminence of these gouty subjects is as notable as their number. They include Milton, Harvey, Sydenham, Newton, Gibbon, Fielding, Johnson, Wesley, Landor, W. R. Hamilton and Darwin, while Bacon was of gouty heredity.* It would probably be impossible to match the group of gouty men of genius, for varied and preeminent intellectual ability, by any combination of non-gouty individuals on our list. It may be added that these gouty men of genius have frequently been eccentric, often very irascible—'choleric' is the term applied by their contemporaries—

* Sydenham, the greatest of English physicians, who suffered from gout for thirty-four years, and wrote an unsurpassed description of its symptoms, said in his treatise, 'De Podagra,' that "it may be some consolation to those sufferers from the disease who, like myself and others, are only modestly endowed with fortune and intellectual gifts, to know that great kings, princes, generals, admirals, philosophers and many more of like eminence have suffered from the same complaint, and ultimately died of it. In a word, gout, unlike any other disease, kills more rich men than poor, more wise than simple." And another ancient (Father Balde) called gout *Dominus morborum et morbus dominorum.*

and occasionally insane. As a group, they are certainly very unlike the group of eminent consumptives. These latter, with their febrile activities, their restless versatility, their quick sensitiveness to impressions, often appear the very type of genius, but it is a somewhat feminine order of genius. The genius of the gouty group is emphatically masculine, profoundly original; these men show a massive and patient energy which proceeds not only 'without rest,' but 'without haste,' until it has dominated its task and solved its problem.

This association of genius and gout cannot be a fortuitous coincidence. The secret of the association probably lies in the special pathological peculiarities of gout. It is liable to occur in robust, well-nourished individuals. It acts in such a way that the poison is sometimes in the blood, and sometimes in the joints. Thus not only is the poison itself probably an irritant and stimulant to the nervous system, but even its fluctuations may be mentally beneficial. When it is in the victim's blood his brain becomes abnormally overclouded; when it is in his joints his mind becomes abnormally clear and vigorous. There is thus a well-marked mental periodicity; the man liable to attacks of gout is able to view the world from two entirely different points of view; he has, as it were, two brains at his disposal; in the transition from one state to another he is constantly receiving new inspirations, and constantly forced to gloomy and severe self-criticism. His mind thus attains a greater mental vigor and acuteness than the more equable mind of the non-gouty subject, though the latter is doubtless much more useful for the ordinary purposes of life.

It must not be supposed that in thus stating a connection between gout and genius it is thereby assumed that the latter is in any sense a product of the former. All the uric acid in the world will never suffice of itself to produce genius, and it is easy enough to find severe gout in individuals who are neither rich nor wise, but merely hard-working manual laborers of the most ordinary intelligence. It may well be, however, that, given a highly endowed and robust organism, the gouty poison acts as a real stimulus to intellectual energy and a real aid to intellectual achievement. Gout is thus merely one of perhaps many exciting causes acting on a fundamental predisposition. If the man of genius is all the better for a slight ferment of disease, we must not forget that if he is to accomplish much hard work he also requires a robust constitution.

It may be added that the other diseases of the uric acid group are common among our men of genius. Rheumatism, indeed, is not mentioned a very large number of times, considering its prevalence among the ordinary population. But stone, and closely allied conditions, are mentioned seventeen times (five times in association with gout), and as we may be quite sure that this is a very decided underestimate, we must certainly conclude that the condition has been remarkably common.

One other grave pathological state remains to be noticed in this connection—insanity. To the relationship of insanity with genius great importance has by some writers been attached. That such a relationship is apt to occur cannot be doubted, but it is far from being either so frequent or so significant as is assumed by some writers, who rake together cases of insane men of genius without considering what proportion they bear to sane men of genius, nor what relation their insanity bears to their genius. The interest felt in this question is so general that we may be fairly certain that the national biographers have rarely failed to record the facts bearing on it, although in some cases these facts are dubious and obscure. They may often have passed over gout without mention, but they have seldom failed to mention insanity whenever they knew of its occurrence. It is, therefore, possible to ascertain the prevalence of insanity among the persons on our list with a fair degree of approximation to the truth as it was known to the eminent man's contemporaries. We thus find that twenty-one were certainly insane at some period during the prime of their lives; that thirteen others were probably, but not certainly, insane at some period earlier than old age, and that in eleven further cases mental decay set in before death took place in old age. It may be added that at least nine committed suicide, and that at least fifteen were to a very high degree eccentric, although there is no clear reason to suppose that they were actually insane. It also appears that in seven cases (two fathers and five mothers) one of the parents became insane, and that in eight cases one or more of the children were insane. So that the insanity of the ascendants and descendants, so far as can be seen, was about equal and by no means excessive. If we include every possible case of insanity which may be inferred from the data supplied by the national biographers, and even if we include that decay of the mental faculties which is naturally liable to occur before death in extreme old age, we find that the ascertainable incidence of insanity among our 902 eminent persons is nearly 5 per cent.

It is certainly a high proportion. I do not know what is the number of cases among persons of the educated classes living to a high average age in which it can be said that insanity has occurred at least once during life. It is doubtless lower, but at the same time it can scarcely be so very much lower that we are entitled to say that there is a special and peculiar connection between genius and insanity. The association of genius with insanity is not, I believe, without significance, but in face of the fact that its occurrence is only demonstrable in 5 per cent. cases, and that it is only in 1 per cent. cases demonstrable in the parents puts out of court any theory as to genius being a form of insanity.

While I cannot compare with any precision the liability of these

persons of genius to insanity with the similar liability of corresponding normal classes, there is one comparison which it is interesting to make. We may compare the liability of persons of genius to insanity with the similar liability of their wives or husbands. It is noted by the national biographers that in fourteen cases the wives or husband (there is only one case of the latter) became insane. We may be fairly certain that this is a decided underestimate, for while the biographers would hold themselves bound to report the insanity of their subjects, they would not consider themselves equally bound to give similar information concerning the wives, while in other cases it may well be that the record of the fact has been lost. If now, in order to make the comparison reasonably fair, we omit the cases of senile decay, and only admit two-thirds of the doubtful cases of insanity, we find that the proportion of cases of insanity among the persons of genius is 3.3 per cent. Among the conjugal partners, on the other hand (I have not made any allowance for second marriages), it is 2.4. Thus we see that on a roughly fair estimate the difference between the incidence of insanity on British persons of genius and on their wives or husbands is less than one per cent. When we bear in mind that the data on which one of our groups is based are much more complete than those on which the other is based, it is not hazardous to assert that British men of genius have probably not been more liable to insanity than their wives.

At the first glance it might seem that this may be taken to indicate that the liability of genius to insanity is exactly the normal liability. That, however, would be a very rash conclusion. If the wives of men of genius were chosen at random from the general population it would hold good. But there is a well-recognized tendency—observed among all the mentally abnormal classes—for abnormal persons to be sexually attracted to each other. That this tendency prevails largely among persons of eminent intellectual ability many of us may have had occasion to observe. What we see, therefore, is not so much the conjunction of an abnormal and a normal class of persons, but the presence of two abnormal classes.

With regard to the significance of insanity, it must be pointed out that, although there may be an unusual liability to insanity among men of genius, there is no general tendency for genius and insanity, even when occurring in the same individual, to be concomitant. Just as it is rare to find anything truly resembling genius in an asylum, so it is rare to find any true insanity in a man of genius when engaged on his best work. The simulation of it may occur—the 'divine mania' of the artistic creator, or a very high degree of eccentricity—but not true and definite insanity. There seem to be only two certain (and two or three possible) cases—mostly poets—in which the best work was done during the actual period of insanity. Periods of insanity may alternate with

periods of high intellectual achievement, just as gout may alternate with various neurotic conditions, but the two states are not concomitant, and genius cannot be accurately defined as a disease.

It must also be pointed out, in estimating the significance of the relationship between genius and insanity, that the insane group is on the whole not one of commanding intellectual preeminence. It cannot compare in this respect with the gouty group, which is about the same size, and the individuals of greatest eminence are contained in the 'probable and doubtful' sections of the insane group. Among poets and men of letters, of an order below the highest, insanity has been somewhat apt to occur; it has been especially prevalent among antiquarians, but the intellectual eminence of antiquarians is often so dubious that the question of their inclusion in my list has been a frequent source of embarrassment.

If we turn from insanity to other grave nervous diseases, we are struck by their rarity. It is true that many serious nervous diseases have only been accurately distinguished during the past century, and that we could not expect to find much trace of them in the dictionary. But that cannot be said of epilepsy, which has always been recognized, and in a well-developed form cannot easily be ignored. Yet epilepsy or an epileptoid affection is only mentioned twice by the national biographers—once as occurring in early life (Lord Herbert of Cherbury), once in old age (Sir W. R. Hamilton), never during the working life. Although some of the most famous men in the world's history have been epileptics, it cannot be said that the lives of British men of genius favor the belief in any connection between genius and epilepsy, nor, so far as can be seen, do they furnish a single shred of evidence in support of the theory that genius is an epileptoid neurosis.

While, however, grave nervous diseases of definite type seem to be rare rather than common among the eminent persons with whom we are dealing, there is ample evidence to show that nervous symptoms of vaguer and more atypical character are extremely common. The prevalence of eccentricity I have already mentioned. That irritable condition of the nervous system which, in its Protean forms, is now commonly called neurasthenia, is evidently very widespread among them, and probably a large majority have been subject to it. Various definite forms of minor nervous derangement are also common, especially stammering or stuttering; this is noted as occurring in nine cases.* In seventeen other cases we are told that the voice was shrill,

* Even this means a higher proportion than is found among the general population, and it must be remembered that the real occurrence must be reckoned as at least double that which may be ascertained from the 'Dictionary.' The normal occurrence of stuttering and stammering among adults is much below one per cent., and even among children it is under one per cent.

weak or small. Short-sight, another condition occurring on a basis of hereditary nervous defect, is noted as occurring in an extreme degree thirteen times; and in a certain number of cases the other senses are defective or absent. Convulsive or twitching movements of the face, etc., are unusually frequent, and are noted in nine cases.

A condition to which I am inclined to attribute considerable significance from the present point of view is clumsiness in the use of the hands and awkwardness in walking. A singular degree of clumsiness or awkwardness is noted many times by the national biographers, although they have certainly regarded it merely as a curious trait, and can scarcely have realized its profound significance as an index to the unbalanced make-up of the nervous system. This peculiarity is very frequently noted as occurring in persons who are tall, healthy, robust, full of energy. As boys they are sometimes not attracted to games, and cannot, if they try, succeed in acquiring skill in games; as they grow up all sorts of physical exercise present unusual difficulties to them; they cannot, for instance, learn to ride; even if fond of shooting, they may be unable to hit anything; they cannot write legibly; in walking they totter and shuffle unsteadily; they are always meeting with accidents. Priestley, though great in experiment, was too awkward to handle a tool; Macaulay could not wield a razor or even tie his own neckcloth; Shelley, though lithe and active, was always tumbling upstairs or tripping on smooth lawns. It would be easy to fill many pages with similar examples. It is noted of thirty-four eminent men on our list that they displayed one or more such inaptitudes to acquire properly the muscular coordinations needed for various simple actions of life. In numerous cases this clumsiness was combined with voice defect.

The existence of all these nervous incoordinations and defects is not evidence of disease, but it is yet in harmony with the evidence that we have obtained regarding the diseases most prevalent among British persons of genius. We have seen that the national biographers have revealed the special frequency of consumption, of spasmodic asthma, of angina pectoris, of gout, among persons of high intellectual aptitude. To a large extent these pathological conditions are closely related, and even interchangeable among themselves; they are all closely related to various neurotic conditions. A man of genius may indeed be, as it were, highly charged with nervous energy, but that energy is apt to be ill-balanced, and by no means always equably and harmoniously distributed throughout the organism.

THE INTELLIGENCE OF MONKEYS.

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A GOOD test of the intelligence of any animal is its ability to learn to do a thing by being shown it or by being put through the requisite movements. Human adults would learn readily in either of these ways, because we thus get ideas of what to do and how to do it and modify our actions in accordance with these ideas. If the reader had never seen a glass or a faucet, he would nevertheless learn how to get a drink by turning a faucet and holding the glass beneath it, if he saw some one else do it, or if some one took his hands and put them through the movements. The intelligence required in such cases is not of a very advanced sort; it is not the power of abstract reasoning or of seeing the relationships of facts, but is simply the capacity to have ideas and to progress from the idea of doing a thing to the act itself.

A study which I made four years ago of the mental powers of dogs, cats and chicks showed that these animals did not, at least not habitually, learn from this sort of tuition. They learned only in the following manner: If in any situation their own impulses led them to do something which brought desirable results, they would, when put in that situation again, do that particular thing rather than anything else. If for instance a kitten was shut up in a box from which it could escape only by turning around a button which held the door, it would claw and bite and pull and squeeze at random as its instinctive impulses led it to do. If by chance it made a pull at the button and so secured freedom and food, it would, the next time it was put in that box, be likely to make that particular movement earlier than in its first trial. After enough trials it would pull the button around as soon as put into the box. It had learned by the selection of one of its own impulses. If you showed it how to get out by putting it in the box and turning the button for it, thus letting it out, it learned no more quickly than when by itself. So also if you took its paw and with it pulled the button round. And any acts which it failed to learn by means of the selection of chance successes from its own impulsive activities, it could never be taught by example or by being put through the movements, scores of times.

During 1900 I was engaged in investigating the mental capacities of monkeys and included in my experiments a number bearing upon this question. The monkeys are quicker to learn and learn more

things than do the dogs and cats. Considering this and also their close relationship physically to man, it seemed of special interest to discover whether they could learn from example or from being put through certain movements, whether they would manifest the capacity so evident in man and so lacking in the lower animals in general.

I had one monkey for over a year and two others for about four months. All three were of the genus Cebus. The general method of the experiments was as follows: Boxes about a foot square were made with small doors held by all sorts of contrivances. For instance, one was held by a hook, another by a bar, another by a bolt, another by a wire fastened firmly at one side and wound round a nail at the other. A bit of food was put inside such a box and the door of the box left open. The box was then put inside one of the large cages containing a monkey. It would come down, reach into the box and get the food. After this had been repeated a few times the box would be put into the cage, with its door shut. The monkey would try to get in as before. He might chance to operate the simple mechanism that held the door. If, however, he did not succeed of his own impulsive activity I would show him or put him through the movement a few times and then leave him to himself again to see if he had profited by the tuition.

The general result was that they did not profit by the tuition, that they did not gain and use ideas of how to open the doors, but learned only by a process of selection from their own impulses. The meaning and value of this general fact will appear in the details of the experiments.

In order that such experiments shall be valid tests of the workings of an animal's mind it is necessary that he surely desire to get into the box, that he be not disturbed by the surroundings in any way that will alter his mental efficiency, and that the experimenter be able to handle him easily without frightening him or taking his attention away from the box. In all cases it is further necessary to make sure that the monkey sees you perform the acts you expect him to imitate, and sees and feels himself make the movements you put him through. These desiderata were obtained by testing the monkeys when hungry and using bits of food of which they were especially fond as the attraction; by experimenting with them after they were quite used to their habitat and to my presence; by getting them into the habit of coming to me and enjoying being handled, having their paws taken, etc.; by showing them the act or putting them through it only when they were attending to the box.

A sample of one of the experiments on the influence of example is the following: The box used was arranged so that the door opened when a brass lever was depressed about an eighth of an inch. The monkey could reach this lever by putting his hand through a hole about an inch

square at the right side of the front of the box. The door was at the left side of the front. On January 12th I put this box in No. 3's cage, the door of the box being open. I put a bit of food in the box. No. 3 reached in and took it. This was repeated three times. I then put in a bit of food and closed the door. No. 3 pulled and bit the box, turned it over, fingered and bit at the hole where the lever was, but did not succeed in getting the door open. After ten minutes I took the box out. The monkey having failed by his own impulsive efforts to depress the lever, I began the tuition. I took No. 3 out and let him sit on my knees (I sitting on the floor with the box in front of us). I would then put my hand out toward the box and when he was looking at it would insert my finger and depress the lever with as evident a movement as I could. The door, of course, opened, and No. 3 put his arm in and took the bit of food. I then put in another, closed the door and depressed the lever as before. No. 3 watched my hand pretty constantly, as all his experiences with me had made such watching profitable. After ten such trials he was put back in the cage and the box put in with a large piece of food in it and its door closed. No. 3 failed in the course of five minutes to get the door open. His behavior was just the same as it had been before he had seen me open the door ten times. He had not profited at all by my example. Later I showed him 15 times more and then tried him by himself. He failed as before.

The two monkeys, No. 1 and No. 3, were given a number of such chances to learn acts from seeing me. Other boxes were used, the doors of which could be opened by pulling up a bolt, pulling out a plug, pushing a bar back into a slot, unwinding a wire and pulling a loop off from a nail. I had also certain pieces of apparatus arranged which would throw a bit of food down a chute into the cage when some simple mechanisms were operated; when for instance a nail was pulled out of a hole or a loop pulled off a nail or a bar pushed in. These could be set up outside the cages so that the monkeys could reach them through the wire netting and could easily see me operate them.

No. 1 had in all eight chances to learn from seeing me. In seven of the cases he failed utterly after seeing me operate the mechanisms 21, 5, 10, 4, 15, 40 and 15 times respectively. He did succeed in one case where the act required was to pull a wire loop off a nail. This must, I think, have been an accident. The other monkey failed utterly to learn to do the same thing though he had continued tuition.

No. 3 had seven chances to learn from seeing me. In five out of the seven he failed after seeing me operate the mechanisms 40, 30, 25, 5 and 30 times respectively. In the case of the other two, although he succeeded in getting the door open, it was not by doing as I had shown him. I opened a door 25 times by pulling a bolt up, but he opened it by pulling and pushing at the door itself until he worked the bolt up out

of place. In the other case I pulled a hook out from a catch but he yanked at the bar to which the hook was attached and so jerked the latter free.

It might be that although the monkeys did not succeed after tuition where they had previously failed, yet they *attempted* acts which they had not previously attempted. This is not the case, however. There were no signs that the monkeys tried more after tuition to do the things they saw me do than they did before. Their behavior was unmodified by the tuition save that in general they tried less.

It may be objected that the acts I failed to teach the monkeys were not consistent with their make-up, that a monkey might be very intelligent and still not manifest his intelligence by depressing levers, unwinding wires or pulling off loops, that monkeys might be able to learn to do certain things from seeing them done and still be unable to learn the particular acts needed in these experiments. But as a matter of fact, these particular acts were quite natural for the monkeys, quite in accord with their interests and propensities. They learned by the typical animal method acts of the same general sort, *e. g.*, to open boxes and operate the mechanisms throwing food into their cages by pulling bars around, unhooking hooks and pulling at strings. And often the very same act with which I tested one monkey in the experiments just described had been learned by another through the repetition and selection of a chance success. Thus No. 1 learned of himself to unwind a wire though No. 3 failed to do so after seeing me do it 30 times.

The systematic experiments designed to detect the presence of ability to learn from human beings are thus practically unanimous against it. So too was the general behavior of the monkeys, though I do not consider the failure of the animals to imitate common human acts as of much importance save as a rebuke to the story-tellers and casual observers. The following facts are samples: The door of No. 1's cage was closed by an iron hoop with a slit in it through which a staple passed, the door being held by a stick of wood thrust through the staple. No. 1 saw me open the door of his and other cages by taking out sticks hundreds of times, but though he escaped from his cage a dozen times in other ways he never took the stick out and to my knowledge never tried to. I myself and visitors smoked a good deal in the monkeys' presence but a cigar given to them was always treated like anything else.

The following is a sample of the tests of the monkey's ability to learn to do a thing from being made to do it: A box was arranged with its door held closed by a bar of wood held in position in a slot. When it was pushed back an inch and a half further into this slot the door could be opened. It was fastened so that it could not be pulled out from the slot altogether. The only way to get the door open was

thus to push or pull the bar back. It worked very easily, a pressure of perhaps 15 grams being sufficient. On January 4, 1901, this box was put in No. 1's cage. He failed to get in in 5 minutes, though he was active in trying to get in for about 4 minutes of the time and pulled and pushed the bar a great deal, though up and down and out instead of back. In his aimless pushings and pullings he nearly succeeded. He failed in 5 minutes in a second trial also. I then opened the door of the cage, sat down beside it, held out my hand, and when he came to me took his right paw and with it (he being held in front of the box) pushed the bar back (and pulled the door open in those cases when it did not fall open of itself). He reached in and took the food and went back to the top of his cage and ate it. I put him through the act thus 10 times. I then let him try alone. He failed to get in. In this and the two following days No. 1 was put through the act 80 times and given frequent opportunities to open the box himself. He never derived the slightest profit from the tuition.

No. 1 had eight such tests and No. 3 had six. Their behavior was in some cases ambiguous but the verdict would surely be that they had no general capacity to acquire these simple habits by seeing and feeling themselves make the movements and get food thereby.

The theoretical importance of the failure of the monkeys to learn from example or from being put through movements consists in the testimony it bears to their lack of a general fund of ideas. Adult human beings learn to do things by getting ideas of the circumstances and of the acts required and then proceeding to act upon these ideas. We think of where we are going, and so go; we have an idea of what we wish to do and so do it. Rarely if ever do monkeys learn in this way.

The behavior of the monkeys apart from these specific experiments seemed also to show their inability to acquire and use ideas of objects or acts. In getting them so that they would let themselves be handled, it was of almost no service to *take* them and feed them while holding them or otherwise make that state pleasant for them. By far the best way is to wait patiently till they do come near, then feed them; wait patiently till they do take hold of your arm, then feed them. If you do take them and hold them partly by force you must feed them only when they are comparatively still. In short in taming them one comes unconsciously to adopt the method of rewarding certain of their impulses rather than certain *conditions* which might be associated in their minds with ideas, had they such.

Monkey No. 1 apparently enjoyed scratching himself. Among the stimuli which served to set off this act of scratching was the irritation from tobacco smoke. If anyone blew smoke in No. 1's face he would blink his eyes and scratch himself, principally in the back. After a time he got in the habit of coming to the front of his cage when anyone

was smoking and making such movements and sounds as in his experience had attracted attention and caused the smoker to blow in his face. He was often given a lighted cigar or cigarette to test him for imitation. He formed the habit of rubbing it on his back. After doing so he would scratch himself with great vigor and zest. He came to do this always when the proper object was given him. I have recounted all this to show that the monkey enjoyed scratching himself. *Yet he apparently never scratched himself except in response to some sensory stimulus.* He did not with all his experiences of scratching ever get the idea of that act and use it to arouse the delightful act. He was apparently incapable of thinking 'scratch' and so doing. Yet the act was quite capable of association with circumstances with which as a matter of hereditary organization it had no connection. For by taking a certain well-defined position in front of his cage and feeding him whenever he did scratch himself I got him to scratch always within a few seconds after I took that position.

The fact that monkeys do not possess the human type of ideas must not be taken as evidence that they are no nearer relatives to us mentally than are the other lower animals. On the contrary they occupy an intermediate position in every main psychological feature between mammals in general and the human species.

The essentials in an inventory of an animal's mental capacities are its sense powers, the kinds of movements it can make and their delicacy, complexity and number, its instincts or the sum of those tendencies to feel and act which it has apart from experience or learning, and its methods of learning or of modifying its behavior to suit the multitudinous circumstances of life. In each of these respects the monkeys show kinship with man.

In point of sense powers they rely little on smell and much on vision. They possess the power of clear, detailed vision which is absent, for instance, in dogs and cats and is so important a possession of man. A monkey will notice a hair on your hand or a pin six feet off. He thus resembles man in what has been universally recognized as the most intellectual of the senses.

In their motor equipment monkeys possess first of all the muscular coordinations necessary to sustain an upright position and consequently the free use of the fore-limbs. The movements of these fore-limbs are more in number and suited to more complex and varied tasks than are those of lower animals. The attractiveness of the monkey cage in a zoological garden is largely due to the similarity of the monkeys' movements and our own. The monkey not only has a body like a man's, but he also uses it like a man.

Our native tendencies are so metamorphosed by the education of a civilized environment that in adult age they seldom appear in recogniz-

able form. But if we take human beings at from 6 months to 3 years of age or later, we find plenty of traits that appear in the monkeys. In fact the human instinct which is perhaps of prime importance in human mentality, the instinct which perhaps is the real cause of many of our most boasted powers, has its clear prototype and homologue in the monkey. I refer to the instinctive enjoyment of physical and mental activity in general, to the tendencies to act and feel as much as possible, regardless of any ulterior practical considerations, which we sometimes call destructiveness or constructiveness and curiosity.

Even the casual observer, if he has any psychological insight, will be struck by the general, aimless, intrinsically valuable (to the animal's feelings) physical activities of a monkey compared with the specialized, definitely aroused, utilitarian activities of a dog or cat. Watch the latter and he does but few things, does them in response to obvious sense presentations, does them with practical consequences of food, sex-indulgence, preparation for adult battles, etc. If nothing that appeals to his special organization comes up, he does nothing. Watch a monkey and you cannot enumerate the things he does, cannot discover the stimuli to which he reacts, cannot conceive the raison d'être of his pursuits. Everything appeals to him. He likes to be active for the sake of activity.

The observer who has proper opportunities and takes proper pains will find this intrinsic interest to hold true of mental activity as well. No. 1 happened to hit a projecting wire so as to make it vibrate. He repeated this act hundreds of times in the few days following. He could not eat, make love to or get preliminary practise for the serious battles of life out of that sound. But it did give him mental food, mental exercise. Monkeys seem to enjoy strange places; they revel, if I may be permitted an anthropomorphism, in novel objects. They like to have feelings as they do to make movements. The fact of mental life is to them its own reward.

Finally in their method of learning, although monkeys do not reach the human stage of a rich life of ideas, yet they carry the animal method of learning by the selection of impulses and association of them with different sense impressions, to a point beyond that reached by any other of the lower animals. In this, too,they resemble man; for he differs from the lower animals not only in the possession of a new sort of intelligence but also in the tremendous extension of that sort which he has in common with them. A fish learns slowly a few simple habits. Man learns quickly an infinitude of habits that may be highly complex. Dogs and cats learn more than the fish, while monkeys learn more than they. In the number of things he learns, the complex habits he can form, the variety of lines along which he can learn them, and in their permanence when once formed, the monkey justifies his inclusion with man in a separate mental genus.

COCAINE ANALGESIA OF THE SPINAL CORD.

By SMITH ELY JELLIFFE, M.D., Ph.D.

THERE are surgeons living to-day who remember the fascination and horrors of necessary operations, when speed was as great a requisite as skill to shorten the mortal agony, and when a famous surgeon would remove a limb in eleven minutes. There are many who remember how slowly the boon of chloroform worked its way against prejudice. To give it to ease the pain of childbirth was not only unsafe, according to the family doctor, but sacrilegious, according to the preacher, for did not the Holy Writ say, 'In sorrow shalt thou bring forth children,' and who of Adam's daughters should presume to escape the curse? Had it not been for the wit of Dr. Simpson, who insisted that the Lord performed the first surgical operation under anæsthesia when He caused Adam to fall into a deep sleep and took a rib from his side, and the courage of Queen Victoria, who set the example to the women of her empire by trusting her physician to give her chloroform at the birth of one of her children, it is quite possible that the ease from pain of all kinds might have been longer delayed.

Soon after chloroform came ether, the safer anæsthetic, and the one more frequently used, to produce unconsciousness in pain; and then cocaine, that peculiar drug that, injected into the tissues, benumbs the nerves and abolishes sensation of pain, and that gives the last word of the century on anæsthesia.

When the anæsthetic properties of this alkaloid of coca were discovered, and it had been demonstrated that abscesses could be opened and slight, but otherwise very painful, operations could be performed without pain, under its influence, it was considered the one thing necessary to complete the series of anæsthetics. The nerves, however, quickly recovered from the effects of the drug, and hence operations had to be accomplished in a comparatively short time. Until recently, only minor operations of the external parts of the body could be performed, and cocaine has been classed merely as a local anæsthetic; but its future has suddenly opened along new and startling lines in the discovery that when it is injected into the spinal cord it causes a total loss of sensation to pain below the point of puncture, so that most elaborate and difficult operations may be carried on while the patient chats pleasantly with the surgeon and attendants.

This discovery, like so many in medical science, did not flash into existence like a new star in the firmament, but was the result of

researches of different men of various nationalities, which finally culminated in a practical result.

It was to an American, a well-known physician in New York, that we owe the first suggestion of the idea. Dr. J. Leonard Corning, in 1885, discovered that frogs, those benefactors to the human race on whom so many of the experiments for the good of man have been tried, could get the characteristic reaction of strychnine from very small solutions of the drug if it were injected into the spinal cord. He then bethought him to try the effect of cocaine; he accordingly experimented on dogs, injecting the anæsthetic between the superior processes of the vertebræ, where the numerous minute vessels would carry it to the cord. After a few minutes the dog lost all sensation in its hind legs; it could be pinched and pricked and touched with an electric brush without knowing it, but the same treatment applied to its fore legs brought forth yelps and howls.

As there were no evil effects seen in the various dogs on which Dr. Corning experimented, he tried the effect upon one of his patients who had for some time suffered from spinal weakness; injecting sixty drops of a 3 per cent. solution of cocaine into the tissues about the spine, between the eleventh and twelfth dorsal vertebræ. For the space of half an hour the man had absolutely no sensations of pain in his lower limbs; electricity and pin pricks were alike unnoticed, and Dr. Corning might have amputated a leg had his patient needed to lose one of those members, and at one stroke have taken all the fame of the discovery of a new form of anæsthesia; but in an hour or more the patient arose and walked home, with no unpleasant after-effects, except for a slight headache and dizziness. Dr. Corning reported on the frog, and dog, and man, to his medical colleagues, and threw out a general hint as to the possibility of extending the usefulness of cocaine in operations; but there the matter dropped. His experiments were, however, the germ of a new idea.

Some years later, the German investigator, Quincke, devised a method of puncturing the membrane surrounding the cord, so that he might draw out a few drops of the spinal fluid, to see whether, in such a disease as spinal meningitis, for instance, there were any bacteria present, or whether he could discover any characteristics that would help to diagnose certain obscure cases of disease by observing the pressure of the fluid of the spinal cord.

To penetrate to the very marrow of one's backbone would have seemed, fifteen years ago, to toy with the seat of life. Dr. Corning did not go nearer than the nerve tissues near the cord; but, once Quincke had shown that the needle of a hypodermic syringe could be thrust easily and painlessly and accurately into the space surrounding the spinal cord, and that a few drops of the cerebro-spinal fluid might

be drawn out, it occurred to Dr. Bier, of Kiel, who was the true genius of the discovery, that a few drops of a cocaine solution might be put in, to produce local anæsthesia on a large scale. He worked out the technique of the injection, operated on conscious patients, and reported his success. Although the operations and experiments performed by Bier were published and commented on with interest, they aroused no special excitement beyond a small circle of investigators, and might have remained merely scientific experiments, had it not been for the International Medical Congress, which met at the Paris Exposition. The benefit of the interchange of thought of the ablest scientific men of all countries that is offered by these congresses, which have come into fashion in the last twenty-five years, is incalculable. Every medical journal in every language tells the physician and surgeon of something new, but every day's experience teaches him that it is better to pay attention to the workings of old laws, instead of trying to apply every new remedy; therefore, it was not surprising that even so great a discovery should meet tardy recognition, and should need the dramatic setting of a world exposition to place it prominently before the medical profession. This it obtained at the Paris clinics held by M. Tuffier, where, with all nations for eye-witnesses, he performed one operation after another on patients who were perfectly conscious and yet who were absolutely insensible to pain below the nipple line. His feats were the talk of the Congress; many of the most famous surgeons of the world were present and saw how comparatively simple it was, after first rendering the point of puncture insensible with a little cocaine, to cause the patient to lean forward, as if scorching on a bicycle, thus straining the vertebræ slightly apart, when it was easy to insert the hypodermic needle until the appearance of a few drops of the spinal fluid showed that the cord had been tapped, and then to attach the syringe and inject the cocaine solution.

The surgeons soon dispersed to their own parts of the planet, glorifying the deeds of Tuffier, almost forgetting Bier, who was Tuffier's authority, and never mentioning Corning, from whom came the original idea. But the question of homage was insignificant in comparison with the test of the idea, and, within a few months, the members of the Congress had, in their own clinics and practice, cocainized the spinal cord for laparotomies, amputations and child-bearing, with a varying amount of success.

But, in spite of the fact that in some operations the patients left the operating table with almost no unpleasant sensations, yet in the majority there was dizziness, nausea and frightful headache, which in some cases lasted as long as eight days, in spite of everything to bring relief.

None of the surgeons who have attempted spinal cocainization seem to be able to agree upon the smallest quantity that will ensure anæsthesia, and they are between the horns of the dilemma, that too strong a solution produces violent poisonous effects on the body, and one too weak gives out before the operation is ended, causing the predicament in which one surgeon found himself, when the anæsthetic effects wore off when he was half through, and, having opened the abdomen, he did not dare to permit the patient to sit up and lean over for a second injection.

In all cases, surgeons feel safer to have chloroform or ether at hand in chance of failure, and, when all is said, they do not see any very great advantage in performing the operation under cocaine over the old method. Moreover, many of them say that there is something rather uncanny in the feeling that the patient is conscious of and perhaps watching every stroke of the knife, for, strange to say, sensations of heat and cold, touch and pressure, are still present, and only pain is absent. The older surgeons, before the days of any anæsthetic, mentioned this eerie feeling, and welcomed the patient's unconsciousness of what was being done to him as much as the patient did himself.

There is not enough yet known of the structure of that most wonderful detail of the human organism, the ganglionic nerve cell, to say what may be the effects of certain drugs upon it. Cocaine certainly has a sufficiently anæsthetic effect to make it valuable in those cases where an operation cannot be performed under ether or chloroform on account of a weak heart, or tendencies to asthma, kidney disease, or other complication. Where death would occur either with or without such an operation, it affords a comparatively safe loophole of escape; and at present it will perhaps be confined to such cases.

But, though cocaine anæsthesia cannot at present take the place of the other anæsthetics, it has given a hint of what may be developed; experiments will be continued, to render the lumbar puncture perfectly harmless, to determine the exact amount and strength of cocaine or any other drug that will produce a definite length of anæsthesia, to administer it in such a way as to lessen the unpleasant after-effects, and, if possible, to discover how the upper part of the body may also be rendered anæsthetic. The ideal, absolutely safe and universally applicable general anæsthetic is not yet discovered; but unquestionably a new way has been pointed out, and within a few years the results of scientific experiment will justify the hope that has sprung up at this method of cocainizing the spinal cord, the hope that anæsthesia is still in its infancy, and that when more is known about the effect of drugs on the nerve cells, it will be possible to banish pain, sensation and consciousness at will, and without danger to life.

THE EVIDENCE OF SNAILS ON CHANGES OF LAND AND SEA.

By HENRY A. PILSBRY, Sc.D.,
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IF we wish to learn the history of any land area, we turn to its geology for a record of changes in the past. The time of its emergence from ocean, the age of its mountains and the details of its growth by successive increments of land elevated from the sea, all this we may expect to learn with reasonable accuracy, besides gaining a knowledge of the plants and animals which lived from time to time upon the coasts.

But we may push our inquiry beyond the shore, and ask, Over this expanse of sea did land once extend? Did an arm of the land reach to this island in the old time, or are the islands of that archipelago but the mountain tops of a sunken continent? To such questions geology gives no definite answer. In some cases, to be sure fjords tell their tale of sunken gorges, or soundings give evidence of a subsided coast, with river valleys and former coast-line indicated by submarine topography, as in the continuation of the Hudson River valley outward from New York Harbor, and the old shore-line, now at the hundred fathom contour. But these are exceptional cases; and the ocean bed, blanketed with modern deposits, usually gives but scant information to the geologist.

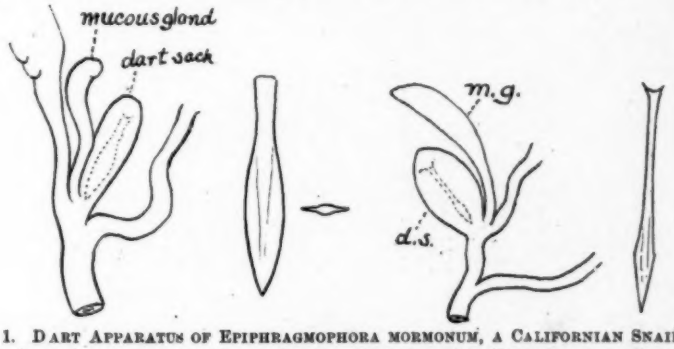
For the solution of the questions we must address ourselves to another and wholly different inquiry: the geographic distribution of living animals and plants.

To the pre-Darwinian naturalist, the relationships of animals among themselves and their distribution over the earth's surface were enigmas, quite insoluble upon the hypothesis of special creation. But the doctrine of descent, of the blood relationship of all the members of a genus and family, fills these problems with meaning. If we find that an island, such as England, has the same species of snails, earthworms, reptiles and fresh water crustacea and fishes as the neighboring continent, it becomes obvious that there has been a land connection in the past, for there is no other means by which any extensive fauna of these animals could have reached an island. If we take another island, and find that while it has different species from the mainland, yet they belong to the same genera, we must conclude that there has been actual land connection here also, though of more ancient date, across which the ancestors of these transformed species emigrated.

It is obvious that animals with feeble powers of travel are of the greatest value in these researches, because they indicate more ancient and more enduring changes of sea and land than freely mobile creatures, such as birds and quadrupeds, which may spread, conditions favoring, with great rapidity. Moreover, the invertebrates have changed much more slowly than higher animals; their evolution has been slow. Almost the whole great drama of mammalian evolution has been acted during Tertiary time, while there has scarcely been generic change in the mollusks! Mammals and birds reflect in their distribution the later earth movements, the invertebrates and fishes the earlier.

The Helix snails are particularly well adapted to show ancient faunal relationships, as they occur under one or another form in almost all lands. But it is only in the present decade that their anatomy has been understood, and the true relationships of the various groups of the family recognized. One of the most interesting developments of the anatomical study of land snails has been the demonstration of a close relationship existing between Helices of the Philippine Islands and eastern Asia and those of California, Mexico and the Greater Antilles.

Years ago Dr. Karl Semper, in his Travels in the Philippine Archipelago, showed that the arboreal Helices of the Philippines are provided with a muscular sack containing a calcareous needle—the so-called 'dart'—and surmounted by a mucous gland, the whole being



1. DART APPARATUS OF EPIPHRAGMOPHORA MORMONUM, A CALIFORNIAN SNAIL. 2. THE DART, ENLARGED. 3. DO. OF CHLORÆA BENGUETENSIS, PHILLIPPINES. THE POSITION OF THE DART IN ITS SACK IS SHOWN BY DOTTED LINES.

an appendage of the reproductive organs. In species of both China and Japan the same peculiarities are found in these organs. It was already known that the Helices of Europe have a similar sack and dart, but the associated mucous gland is split into finger-like tubes and removed from the sack to an adjacent duct. The function of the dart apparatus is not well understood. The snails thrust their darts into one another during the mating time, and hence the dart is believed to be an excitation organ.

Now, when the Helices of California, Mexico and a part of those of the West Indies were examined, it was found that they have *the dart apparatus, agreeing with species of Japan, China and the Philippines,* not with those of Europe or of eastern North America; for, to emphasize this resemblance, the Helices of the middle and eastern United States are anatomically totally unlike the Californian, Mexican and Antillean, having no dart-sack or mucous gland.

We are, therefore, confronted with a group of snails inhabiting both borders of the greatest ocean, but agreeing so closely in anatomy that no hypothesis but that of a common origin, descent from common ancestors, is conceivable. Our American dart-bearing Helices must surely look to distant shores in far-off times for their ancestry.

In the South the Oriental and Occidental members of the great group of dart-bearers are separated by the breadth of the Pacific, the islands of which are barren of related snails. In the North they are parted by many miles of barren coast; for in America the dart-bearers go no further north than Sitka, and in Asia they are not known much to the northward of the Japanese Empire.

MAP SHOWING DISTRIBUTION OF DART-BEARING HELICES IN VERTICAL LINES. CRETACEOUS SEA IN BROKEN HORIZONTAL LINES. BEING ON MERCATOR'S PROJECTION, THE NORTHERN RANGES OF THE DART-BEARERS IN AMERICA AND ASIA APPEAR MUCH MORE SEPARATED THAN THEY REALLY ARE.

We know, however, that in Upper Cretaceous times the Arctic lands were not, as now, clad in the scanty green of mosses, lichens and herbs, with few deciduous trees, except stunted willows and the like, but they bore noble forests of magnolia, beech and birch, with red-woods and pines—such forests as snails love and thrive in, doubtless

with abundance of sheltering windfalls and rotting boles to give them refuge, and decaying leaves to feed the fungi and tender herbage, which are the food of snails.

It is not unlikely, then, that, in the distant past, when a kinder climate allowed the forests of the temperate zone to extend to the Arctic shores in Alaska and Siberia, the snails went with them; and, if we assume a very moderate elevation in the region of Bering Strait, connecting Alaska and Asia by a land bridge, there would be no bar whatever to the spread of forest trees and the emigration of snails from one continent to the other.

The distribution southward of the snails and other inhabitants of the forest would be merely a question of time in the absence of barriers in the form of lofty mountains, deserts or arms of the sea, running across their path.

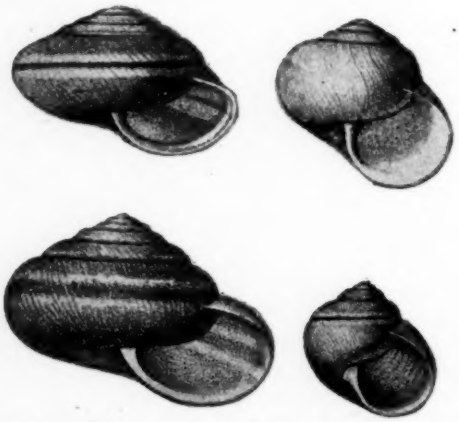
There are good reasons for believing that the dart-bearing snails originated in the Orient, and, if so, their migration was eastward to America. In all probability, the slowly upbuilding land mass in western America had none of the higher land snails before the advent of the Asiatic snails in the later Cretaceous, as it was profoundly isolated in earlier mesozoic and preceding time, so far as existing geological data show. On reaching America, the snails spread southward. Why, then, it may be asked, do we not have the descendants of the Asiatic dart-bearers in eastern North America? There are several reasons. During the Cretaceous period an inland sea extended from the Gulf of Mexico, through the Dakotas and northward to the Arctic Ocean, in the neighborhood of the Mackenzie River.* This would prevent the eastward spread of the snail emigrants from Asia. Since that time, the increasing height of the Rocky Mountains, and the arid conditions of much of the mountain region, with its poverty in deciduous trees, would be, and is to-day, an effectual bar to the eastward distribution of the Pacific slope snails.

In the Far West, however, no barriers prevented the southward spread of the dart-bearing Helices. They pushed south to Mexico, and, perhaps later, to the Andean region of South America. There was also undoubtedly a land bridge connecting an Antillean continent or archipelago with Central America, over which the dart-bearers passed to the Antilles. This connection is shown by many other groups of land snails also, the distribution of which can be explained in no other manner.

* Dawson maps the Cretaceous inland sea as extending to the Arctic Ocean. During the earlier Cretaceous it also reached the Pacific, though an archipelago probably extended north to Alaska; but in the Laramie there was a broad belt of land to the westward of the Cretaceous sea or lakes. See map, which represents the probable extent of the sea at the beginning of the Laramie.

The Helices of the West Indies lend no aid to those who advocate the hypothesis of an 'Atlantis' bridging or partially bridging the Atlantic, for they are not allied to species of Madeira, the Azores, Cape Verde or Canary Islands. Their anatomy is vastly nearer Mexican, Californian and East Asiatic groups.

No generalization based upon the distribution of snails, or of any one group of animals, can be satisfactory unless it is supported by the evidence of animals of other groups, and by that of plants. In extending the data relative to the zoogeography of America and Asia, it may be said that the evidence of the naked snails or slugs fully supports that of the Helices. The West American slugs have their cousins in China and the Himalayas, not in eastern North America. The freshwater crayfish of our Pacific slope belong to the Old World genus *Astacus,* not to the East American genus *Cambarus.* The evidence of fishes seems to strongly favor a former connection of Asia and America. Thus Gunther* deduces a Central Asian origin for the



DART-BEARING HELICES. UPPER FIGURES TWO SPECIES OF EULOTA FROM JAPAN; LOWER FIGURES EPIPHRAGMOPHORA FROM CALIFORNIA.

Cyprinoids, or Carp family, which is also very numerously represented in America. Moreover, he regards the Chinese species of *Catostomus,* or 'sucker,' as a return emigrant from America to Asia. The North American catfishes belong to an East Asian group of the family; and our garpike has a representative in Chinese waters.

Such evidence as the higher vertebrates afford do not strengthen the case stated for the snails, because their evolution has been vastly more recent and rapid, and their means of distribution are far less restricted. Thus the horses have attained their present distribution since the Pliocene, but they are capable of spreading rapidly wherever

* The Study of Fishes, p. 244.

pasturage is to be found. The wide range of such groups as this, and the birds, is limited only by markedly unfavorable physical conditions, and their presence in both the Old and New Worlds merely indicates that up to quite recent times there has been a land bridge over Bering Strait.

The identical species of plants in Japan and the United States, elaborately discussed by Dr. Asa Gray, are also, in many cases, it would seem, comparatively recent emigrants into one continent or the other, not old enough to have become changed by new surroundings; or they are plants which lived in the Tertiary forests of Greenland and British America, which Heer and others have made known. Through stress of climate, this circumboreal flora has been driven southward, many of its species to be changed by the vicissitudes of the march, while others still flourish unchanged in the two continents.

THE BLUE HILL METEOROLOGICAL OBSERVATORY.

By FRANK WALDO.

METEOROLOGY became established on an independent basis about fifty years ago. With the beginning of a systematic study of the atmospheric conditions there arose a demand for more frequent observations than could be made directly, and, as a result, self-registering meteorological instruments came into use. It was speedily found that the exceedingly sensitive and complicated apparatus necessary for furnishing accurate records of the atmospheric conditions required the services of thoroughly trained and skilful persons in its manipulation. Not only this, but proper exposure of the instruments and careful reduction of their records were necessary. In other words, the generally recognized requirements of a good astronomical observatory must be fulfilled in carrying on the work of a meteorological observatory.

It had long been supposed (and unfortunately is still by many) that any one is competent to make meteorological observations who is able to read a barometer scale or hold a measuring stick in a rainfall basin. The importance of having the instruments automatically record their indications became very generally recognized, if we may judge by the number of self-registering instruments constructed and set in operation, although the considerable cost prevented their general introduction. Then it was that the need for this work of well-trained observers began to be felt. Where meteorology was associated with one of the older physical sciences, such as astronomy, the necessary care was given to the meteorograph; but in most cases, after a brief and generally unsatisfactory trial, the self-recording instruments were kept going in only a perfunctory manner or allowed to fall entirely into disuse. While the necessity for meteorological observations continued, and continuous records became more and more imperatively demanded, yet it was not until the true conditions were fully realized and meteorological observatories comparable with those devoted to astronomical research were built, equipped and manned, that anything like satisfactory atmospheric observations were obtained. Nor was it longer deemed sufficient only to keep up the observation of the meteorological elements; the fact was emphasized that the results must be properly worked up and put into such a form as would best serve the purposes for which they were desired.

Observatories of various degrees of excellence and fitness were established at a number of places, but it was reserved for Professor Heinrich

Wild, then director of the Russian Meteorological Service (but now of Zurich), to set us a pattern of what a meteorological observatory should be, in the Pawlowsk Observatory near St. Petersburg.

It has been found by experience that the services of three thoroughly trained and skilled persons are necessary to properly conduct a meteorological observatory. It is the verbal testimony of Dr. Wild that it is better to do entirely without the records of self-registering instruments than to have the records made under the care of untrained and incompetent persons.

A. LAWRENCE ROTCH.

In the United States there long existed an apparent indifference to the demand for numerous continuous atmospheric records, the winds alone receiving the merited attention (except in an experimental way) from our Signal Service organization. A single exception to this indifference was the Central Park Observatory, which was operating unobtrusively along the right lines, but after the stereotyped manner of the older European observatories. For the rest we were mainly content with the observations made at fixed intervals during the day.

Such was the condition of observational meteorology in America at the time when Mr. A. Lawrence Rotch conceived the idea of establishing a meteorological observatory on the Great Blue Hill, near Boston. At first Mr. Rotch intended to use this observatory for special investigations, leaving the regular work to Signal Service observers. As no plan of cooperation with the Signal Service was found feasible, he determined to carry on the entire work under his own direction and at his own expense.

Mr. Rotch was particularly fortunate in his choice of a site for his observatory. Although the summit of the Great Blue Hill is but 635 feet above sea level, yet it possesses many of the characteristics of a mountain. It is the highest point of land in eastern Massachusetts, and offers an unobstructed view for many miles in all directions. This feature has been particularly valuable in prosecuting cloud studies.

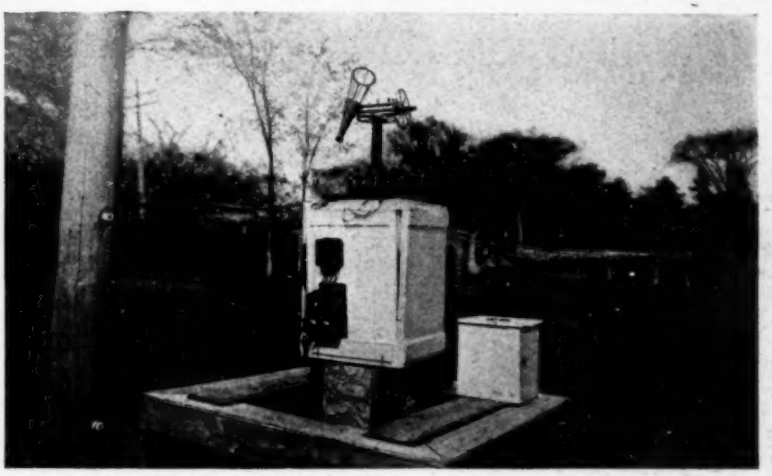
THE VALLEY STATION OF BLUE HILL OBSERVATORY.

The location is so near the coast that the characteristic water and land influences on the atmospheric conditions can be perceived. Moreover the summit of the hill is near that critical altitude at which the diurnal variation of the wind changes from the low level type to the high altitude type. We had meteorological records from the Signal Service stations on Mt. Washington (altitude about 6,000 feet) and on Pikes Peak (altitude about 14,000 feet), but we had none from the lower altitude at which the powerful local influence of the ground surface ceases to be overpoweringly effective. Thus this observatory fitted into a vacancy which it was desirable to fill. Nor is this all. The wonderful success attending the recent extension of the work of the observatory to the exploration of the upper air by means of kites has been in no small part due to the perfect adaptation of this locality for carrying on such

work. As regards location, the Blue Hill Observatory thus occupies a unique position among the meteorological observatories of the world.

Two secondary stations, at altitudes above the sea of 50 and 200 feet, respectively, at the base of Blue Hill bear the same relation to the main observatory that the base stations bear to the higher ones in the most completely planned European mountain observatory systems; while the Weather Bureau station at Boston and the neighboring Harvard Observatory meteorological station offer the advantages of representing the adjacent country.

Just as Dr. Wild set a pattern for Europeans to copy, so Mr. Rotch has given the United States a model observatory which it will be no mistake to use as a pattern in the future development of observational meteorology in this country. It seems to me that every possible precau-



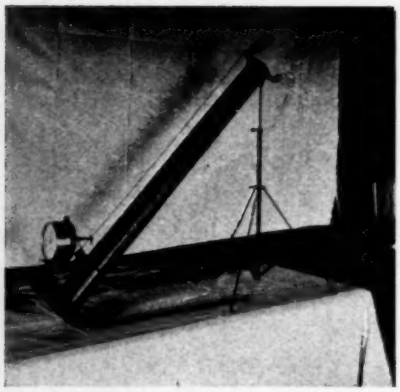
THE STATION FOR CLOUD MEASUREMENTS IN VALLEY, 1896-1897.

tion has been taken in the placing of the apparatus and in its convenient manipulation. The instruments and apparatus are of good construction and well adapted to the work required. The personnel of the staff could not be improved; certainly not in this country and probably not abroad. The true scientific spirit prevails at the observatory, and I have found there the same distinctive atmosphere which marks the Russian observatory at Pawlowsk. There can be no doubt but that the Blue Hill Observatory is the most successfully conducted meteorological observatory in America, and its work will compare favorably with that of European observatories of the highest class.

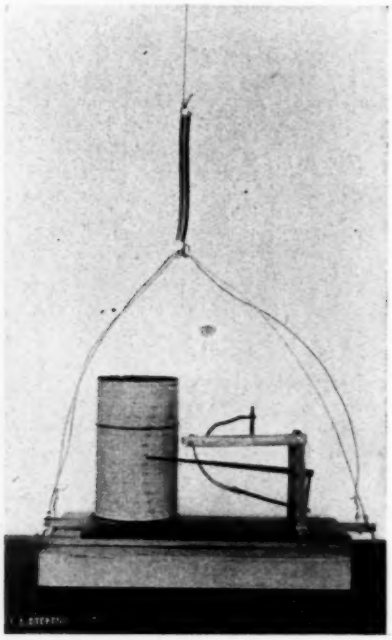
Some may wonder how it was possible for this observatory to have such a good start, reach such a high state of development within a brief space of time, and avoid those errors of organization and management

into which much more ambitious institutions had fallen. The reason is very plain to those who are familiar with Mr. Rotch's numerous visits to the best of the European central and mountain observatories. The care that he has taken to inform himself thoroughly in regard to their equipment, work and general effectiveness is clearly reflected in his printed descriptions of these institutions. By this means the youthful director of this new American observatory was enabled to take what might be termed a 'short cut' to leadership in our observational meteorology.

The regular work of the Blue Hill Observatory is carried on by Mr. Rotch with the assistance of Mr. H. H. Clayton, meteorologist, Mr.



THE POLE STAR RECORDER FOR REGISTERED CLOUDINESS AT NIGHT.

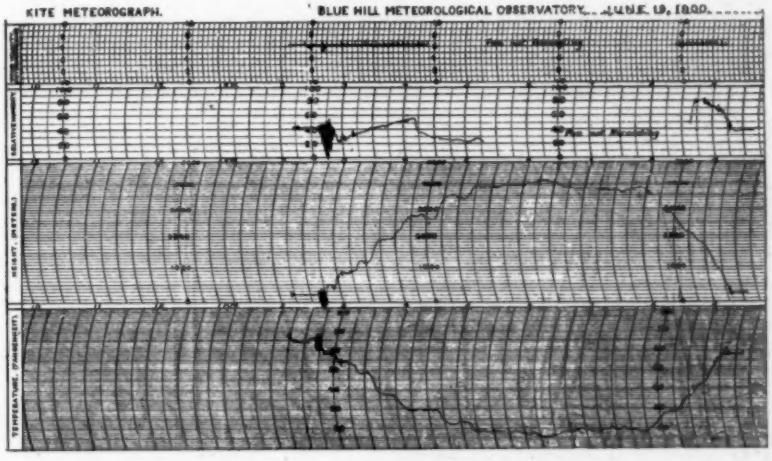


FIRST THERMOGRAPH LIFTED BY A KITE EMPLOYED IN 1894

S. P. Fergusson, mechanician, and Mr. A. E. Sweetland, observer. Not only did Mr. Rotch show excellent judgment in selecting a locality for his observatory, but he has shown equally good judgment in the choice of problems for investigation; he has taken up just those questions concerning which we have been sadly in need of numerical data, and to which every contribution is of distinct value. Moreover Mr. Rotch was exceedingly fortunate in his selection of capable co-workers, for they have responded in a notable manner to the demands which their science has made on them.

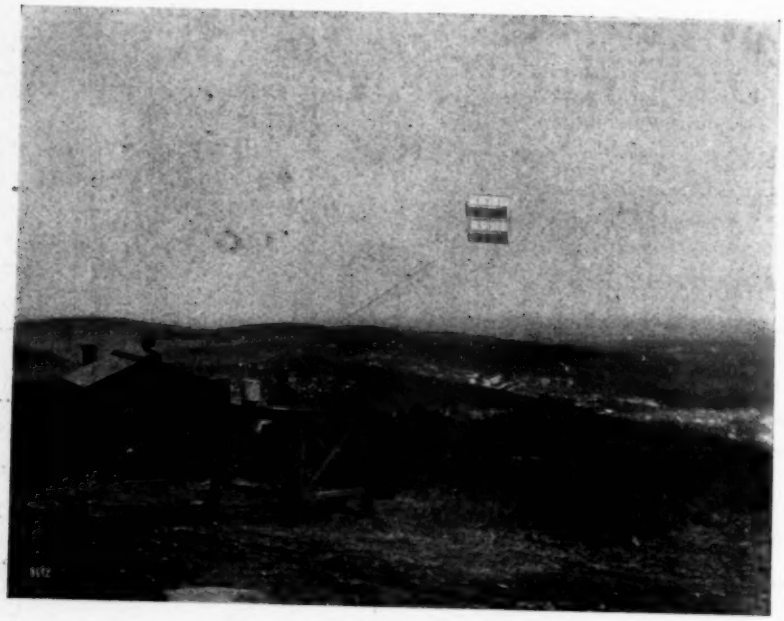
The investigations undertaken at the observatory may be divided into three classes: (1) The routine work of making observations of the local atmospheric conditions both by automatic registration and direct observation; and the reduction and publication of results. (2) The exploration of upper air by means of kites, and (3) Special studies of important topics by the observatory staff and visiting scientists.

The Blue Hill Observatory was established in 1885. Its report for 1886 shows the institution still in its formation period. The annual report for 1887, when it began to appear regularly in the 'Annals of the Harvard College Observatory,' is almost complete, lacking only the hourly values of the relative humidity of the atmosphere, but more than making up for this by the very complete hourly record of cloud observations (from 7 a. m. to 11 p. m.). In the report for 1888 all

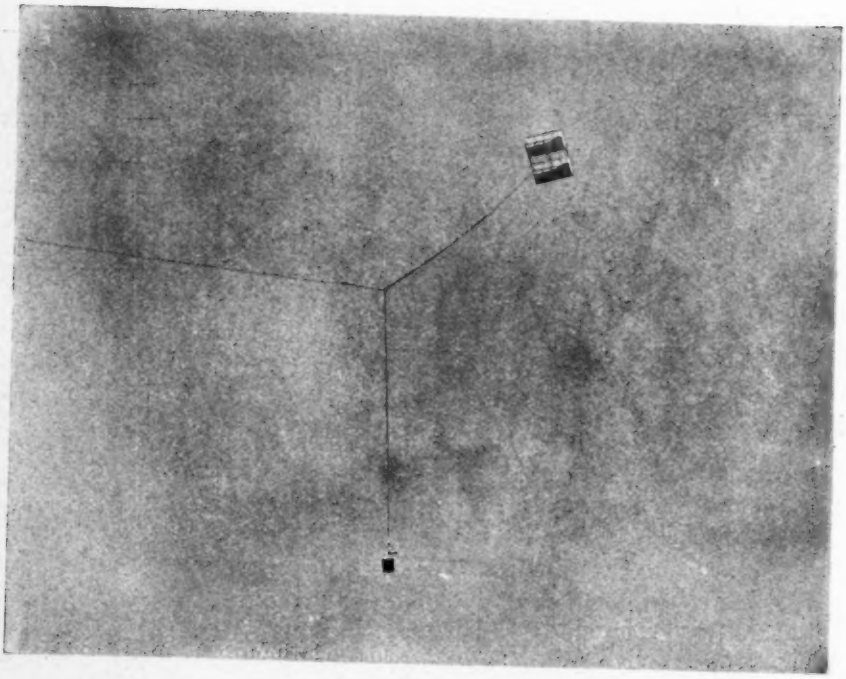


RECORD OF A KITE METEOROGRAPH.

the usual meteorological elements are given; but thereafter hourly values of the precipitation and cloud observations alone are given, the other elements being recorded only in summary. The reason for this long backward step, for it certainly is such, was probably the desire to economize the time required for reducing the observations and the cost of printing, although it may have been thought that the publication of the hourly observations *in extenso* during two or three years was sufficient. This latter is not the case, however, for what we most lack in the study of the air conditions in this country is reliable hourly observations conveniently accessible through print. It is especially desirable to have these records when they are as carefully made as those at the Blue Hill Observatory. It must be remarked, however, that the proper reduction of automatic records is a very laborious task.



STARTING A KITE FLIGHT ON BLUE HILL.



KITE METEOROGRAPH IN AIR.

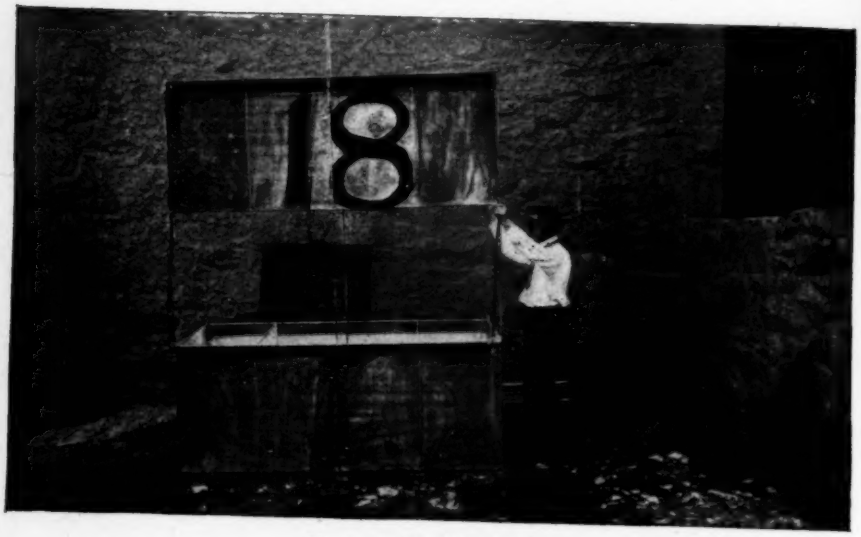
The successive years of continuous or hourly observations have permitted the determination of the diurnal and annual periods of the chief meteorological elements at the Blue Hill Observatory, summaries of which for several years' averages have been published. The main interest in these results centers in the air movements. The constancy of the local amount of wind from hour to hour has been found to be remarkable, and this, in connection with the variability in the hours of maximum and minimum wind, indicates the nearness to the transition altitude where the lower air conditions change to those of the upper air. These observations of wind velocity, coming in as they do at an intervening altitude between those of the ordinary high exposed surface station and the more elevated mountain stations, permitted the discovery of the gradual shifting towards noon of the hour of minimum diurnal wind velocity, with the gradual increase in altitude. Thus the least wind occurs at Boston at 5 a. m., at Blue Hill at 8 a. m., on the Eiffel Tower at 10 a. m. and shortly after noon on Mt. Washington and other similar high altitudes.

Much of the well-earned reputation of the Blue Hill Observatory depends on the special investigations which have been conducted by its scientific staff. Some of these are the natural concomitants of the peculiar location of the observatory, while others have been taken up on account of their intrinsic importance to meteorological science; still others combine these two features.

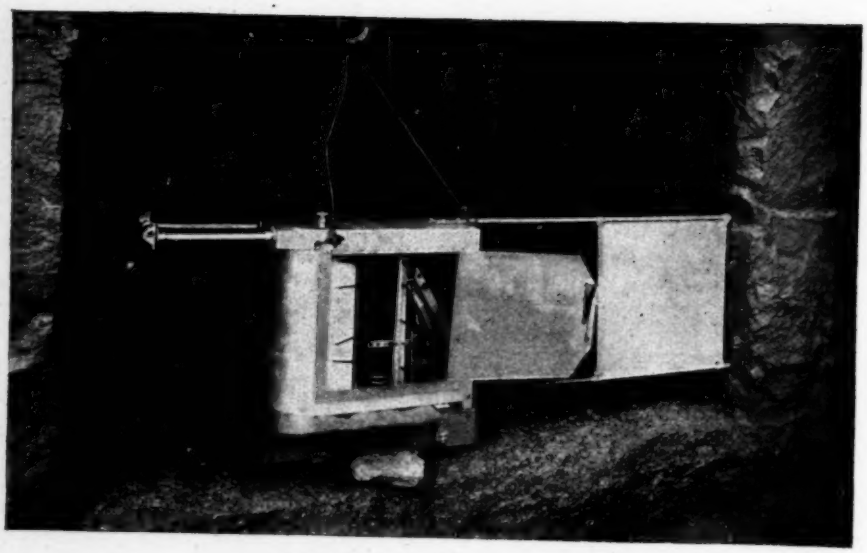
Among the questions taken up for the former reason, the following deserve special mention: The investigation of the normal differences of temperature between the base and the summit of the hill, and between the latter and the neighboring Weather Bureau station in Boston; the investigation of the marked inversions of temperature between the base and the summit stations; experiments on the electrical condition of the atmosphere; and studies of the vertical component of the wind as measured at the observatory.

The Blue Hill series of observations of visibility of more or less distant hills and mountains is very important, although the positive deductions as yet made from the data assembled in regard to this phenomenon are very meager. In general, however, it was found that the summer haze about balanced the winter fogs, so that an annual periodicity is but slightly marked. The diurnal period is also not clearly pronounced.

The location of the Blue Hill Observatory also made it a very desirable place at which to undertake open air experiments on the absolute and relative accuracy of anemometers. These were very much needed in view of the fact that the old errors of observed wind velocities could no longer be neglected when the comparatively recent quantitative study of the winds was widely taken up; and since much of the investiga-



MODIFIED HARGRAVE KITE.



FERGUSSON'S KITE METEOROGRAPH.

tion designed to remedy this defect has been performed under artificial indoor conditions. These Blue Hill investigations showed plainly the necessity for greater uniformity in anemometers both as regards shape and size. The fan or bladed anemometers, with the use of ball bearings, seem to have many advantages over the ordinary cup anemometers now so generally used. There was found to be still much room for improvement in the pressure wind gauges, as those at present in use are not thoroughly satisfactory. The pressure tube anemometers, upon which many hopes have been built, showed need of some further modifications before it will be perfectly adapted to all conditions of wind and weather. Concerning the standardizing and testing of anemometers under artificial conditions, the opinion is advanced that a current of air produced by a blower is more likely to give absolute results than the whirling machine at present in use.

Among the important general meteorological questions taken up are the following: (1) The investigation of the temperature indications of thermometers placed in different kinds of thermometer shelters or screens. (2) The study of special phenomena exhibited by the records of self-registering meteorological instruments; such, for instance, as the dynamic effect of the wind on barograph records. (3) The study of weather predictions, from both the central and local points of view, and the demonstration that the combination of the two methods gave the best results. (4) The study of sudden falls of temperature and their relation to general atmospheric conditions. (5) The study of wave-like oscillations shown in the records of barometric pressures. (6) Studies concerning the periodicity of the weather. (7) The discussion of cloud observations, especially those made at the Blue Hill Observatory. (8) The improvement of meteorological apparatus, especially in the self-registering devices, and adapting the existing instruments to special needs. (9) The study of special cloud forms. (10) Cooperative study of clouds during the International 'Cloud year.'

That the movements of the atmosphere follow on certain laws we all recognize. Some of these laws we know, others remain still to be discovered. No work of the Blue Hill Observatory has exceeded in importance its studies of the actually observed movements of the air, and the so-called dynamic changes which these movements cause the air to undergo. In nearly every phase of this many-sided question this observatory has increased our stock of knowledge.

The study of the upper atmosphere was early begun by the staff of the Blue Hill Observatory. At first it was mainly carried on by means of cloud observations, but since 1894 by means of registering meteorological instruments carried aloft by kites. To this observatory belongs the honor of thus sending up into the air the first continuously

recording meteorological instruments used in this manner. Mr. Eddy, of New Jersey, used his kites in making this first trial. The work has been pushed with such success that records of atmospheric pressure, temperature, relative humidity and wind velocity have been secured by means of kites up to a height of 15,800 feet above the sea.

In a pioneer work of this kind, it was found necessary not only to modify old apparatus and methods so as to fit the novel applications, but also to devise new ones as well; and many of the details of the system as established at Blue Hill have been copied by meteorologists in the prosecution of similar researches both in this country and in Europe.

In this connection the Blue Hill studies of the clouds have led to the consideration of many problems to which these phenomena either directly or indirectly furnish a key. As in other studies carried on there, this work has been undertaken in the light of what has been done by other investigators; and in Mr. Clayton's report on the subject an excellent summary of what has already been accomplished introduces us to the more distinctively Blue Hill work. It has too frequently hap-

EVOLUTION OF THE KITE REEL.

pened that the cloud work which has been done in different parts of the world has had its value much decreased owing to uncertainties in cloud nomenclature, and much of the recognized value of the Blue Hill work is to be attributed to the great care exercised in this preliminary matter. A valuable contribution has been made to the revision of cloud nomenclature, taking into account the elevations of the clouds. The annual and diurnal periodicity of clouds has been carefully studied on the basis of cloudiness at different levels.

In the study of the relation of clouds to rainfall are taken up: clouds preceding rain, clouds between intervals of rain and clouds following rain. The methods and cause of cloud formation were also carefully considered. The most important part of this special investigation is the use to which the cloud observations are put in the study of atmospheric dynamics, taking up in succession the questions: the relation of clouds to cyclones and anti-cyclones, having regard to the altitudes of the cloud levels; the annual and diurnal periods in the winds in general, at various levels as shown by direct observation near the ground and extended upwards to high altitudes by means of the observed cloud movements; the wind movements in cyclones and anti-cyclones from the ground up to the altitude of the highest clouds; the relation of the direction of the cirrus clouds to the existing temperature gradient; the relation between the velocity of storms, and the consequent variability of the weather, to the general movement of the atmosphere as shown by surface wind and cloud observations; the use of cloud observations in weather forecasts; and the frequency of winds from various directions at different heights above the ground, for different hours of the day, shown by wind and cloud observations.

The work of making observations of the atmospheric conditions in the free air by means of kites has been carried out with the success achieved only by the persistent endeavors of the observatory staff, not only in overcoming the difficulties in the mechanical construction of the apparatus employed, but also in the actual work of kite flying.

Experiments were undertaken as to the best forms of kites to use, the best materials for their construction, and the best lines to use for flying them. Special forms of self-recording meteorological instruments had to be so designed or so changed as to be adapted to the demands of kite work. Great care was exercised in so exposing the instruments that their possible errors would be reduced to a minimum. During the year 1897 there were thirty-eight successful kite flights, in 1898 thirty-five, in 1899 twenty-five, and in 1900 twenty-four; the average height above sea level at which records were obtained during the respective years being 7,350 feet, 7,400 feet and 8,450 feet, thus showing constant improvement in the methods employed.

The discussion of the Blue Hill observations has added very materially to our still meager knowledge of the distribution of the meteorological elements in the free air, and their variation with change in altitude. The average increase of wind velocities with increasing altitude was determined chiefly for those altitudes for which we have the fewest data because it is so difficult to make measurements there by means of the clouds. The change in direction of air currents at different levels was also clearly and accurately brought out by the changes in position of the kites as they ascended and descended. Such data as these are

particularly valuable for determining the effects of the friction of the ground on the winds, and for testing quantitatively the theories of the air circulation which have heretofore depended mainly on qualitative generalities.

The decrease (and in the abnormal cases, the increase) in temperature with height above the ground has been carefully studied, and especially in the various phases which occur under different typical atmospheric conditions. The diurnal changes of temperature at different altitudes have been also carefully studied. The determination of the numerical values of these elements is very important in helping to complete the theories of the atmospheric circulation, solar insolation, and the transference of heat from the earth to the air.

The rate of change of relative humidity with change of altitude, due to vertical change of temperature, is very important in connection with the calculation of the heights of clouds by computing the altitude of the dew point temperature under known conditions near the ground; and the Blue Hill observations not only offer data for increasing the accuracy of these calculations, but also a criterion for testing their absolute accuracy.

Until 1886 the only weather map in the United States was printed at the Chief Signal Office in Washington, but in May of that year Mr. Rotch with the assistance of Mr. Cole, the government observer in Boston, began to chart the 7 a. m. reports that were received there, and manifolded the map by the cyclostyle process. This was the origin of the daily weather map that is now issued in great numbers from many of the Weather Bureau Stations throughout the United States.

From 1887 until 1891 local weather forecasts were furnished by the Blue Hill Observatory to the Boston press and announced from the observatory by the display of weather signals. These weather predictions were undoubtedly a considerable improvement over those made in Washington, which depended on the weather map alone, especially for the twenty-four hours immediately succeeding the time of observation, and the demonstration of this, in direct competition with the Weather Bureau predictions, probably had some effect in causing the government service to appoint local forecast officials to supplement the general predictions made at Washington. It must be borne in mind in this connection, that this combined method of making weather predictions has been, in a measure, practically carried out in European countries ever since an international telegraphic exchange of weather observations went into effect. In this country, however, we had learned to rely too much on the general predictions issued from Washington.

There can be no doubt that the work of the Blue Hill Observatory has had a very great quickening influence in the recent developments in observational meteorology in this country. Not only has its thor-

oughly independent attitude and scientific spirit enabled it to make usefulness and not policy its watchword, but it has also permitted it to improve the older traditions of American meteorology, by adding to them the best features of European meteorology.

So far as concerns the regular routine work of observation of the purely local atmospheric conditions made at the Blue Hill Observatory, it is impossible to realize its importance to American meteorology under the light of present conditions alone. One must go back twenty years to the conditions existing in the early eighties to properly appreciate its innovating character. Concerning the extra routine work, such as the studies of the upper air conditions and their application to atmospheric mechanics, no comment seems necessary further than to mention the fact that this work occupies a prominent position in the front line of scientific advance in this direction. We have had a recent example of this in the discoveries attending Mr. Clayton's studies of eclipse meteorology, in which important extensions of Ferrels' cold centered cyclone have in all likelihood been made which will greatly aid in the solution of some hitherto unexplained meteorological problems.

The importance attached by scientists to the work of the Blue Hill Observatory is plainly indicated by the numerous long and appreciative reviews and notices of this work which have appeared in such general scientific journals as 'Nature' and 'Science,' and such special journals as the 'Meteorologische Zeitschrift' and the 'American Meteorological Journal.' Probably not one of the long list of reports and other publications of the observatory has been passed by without printed comment, and frequently a single paper has called forth several reviews. Moreover the confidence with which this work has been received is shown by the fact that the results have been freely used by subsequent investigators.

It is a noteworthy fact that by taking the initiative in the systematic sounding or exploration of the upper air by means of kites, the Blue Hill Observatory has added one more feature to the already long list of American pioneer contributions to meteorology, among which may be mentioned, Loomis' storm and weather maps, Espy's dynamical theories, Ferrels' theories of the atmospheric circulation, and the more complete extension of the application of weather knowledge to practical affairs by the Signal Service and Weather Bureau.

The official publications of the observatory are: the earlier annual reports published separately and the later ones published since 1887 in the 'Annals of the Observatory of Harvard College,' special memoirs and discussions forming an important feature of these reports; monthly and annual summaries of the climatic observations, which were manifolded and distributed locally up to 1896, and some of which the 'U. S. Monthly Weather Review' and the monthly report of the New

England section still publish; occasional Bulletins containing original memoirs that it was desired to publish promptly on special meteorological topics.

In addition to the preparation of these official reports, the members of the observatory staff have published a great number of letters, articles, and reports in the journals of general and technical science both in Europe and America. This individually published material has always been of that high character which bears internal evidence of the earnestness and ability of the authors, and it has always received from scientists both at home and abroad the careful consideration due it.

The high character of the work undertaken and the great amount accomplished by steady application during the fifteen years of its continuance have given the Blue Hill Observatory a position among the best observatories of the world. There are certainly very few even of the great national meteorological observatories which are better known or are held in higher esteem than the private observatory established and maintained by Mr. Rotch on the highest summit of the Blue Hills of Milton.

In closing this article I venture to express the opinion that when the history of meteorology during the latter part of the nineteenth century is written, the Blue Hill Observatory will be assigned the foremost place in American observational meteorology, and this judgment will be based not only on the observations which have been made, but also on their proper discussion and correlation with allied branches of this science of the atmosphere.

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.*

A NATIONAL association for the advancement of science occupies at the beginning of the twentieth century a dominant position. The greatest achievement of the nineteenth century was the progress of science; its most definite tendency was towards the voluntary organization of individuals for the accomplishment of certain ends. The advance of science, the movement that is of the greatest importance for civilization, requires for its guidance the strongest association of individuals. Such an association will certainly arise, and will develop from existing institutions.

The organization of science in America has progressed parallel to the advance of science. Local societies concerned with the whole field of knowledge, and especially with its utilitarian aspects, were first established in Philadelphia, in Boston and in other cities. These societies were modeled on the similar institutions of Europe; the Philosophical Society of Philadelphia following the Royal Society of London, and the Academy of Arts and Sciences of Boston, the Paris Academy of Sciences. As centers of scientific activity increased in number, as the postoffice and railways developed, as general scientific journals were established—'The American Journal of Science' began publication in 1818—the need of a national organization was felt, and here again the older nations had established the precedent. The meetings of German scientific men and physicians began in 1828, and the British Association for the Advancement of Science was established in 1831. An Association of American Geologists and Naturalists was organized in 1840, and became the American Association for the Advancement of Science in 1848.

Fifty years ago the sciences were comparatively undifferentiated. Special societies and special journals were not required. It was pos-

* We reproduce this article from advance sheets of 'Science,' as all our readers will be glad to have brought to their attention the question of the organization of science in America. The American Association for the Advancement of Science meets this year in Denver, further to the west than ever before, and its influence and membership should be greatly increased in the states west of the Mississippi River. Nothing is more gratifying to the conductors of this Journal than its large circulation in the middle and western states, and we hope that many of those who read this article will become members of the American Association. The conditions of membership can be obtained from the permanent secretary, Dr. L. O. Howard, Cosmos Club, Washington, D. C.—EDITOR.

sible for students of science and friends of science to meet together and take a common and intelligent interest in the scientific progress of the day. Somewhat later, however, the need became apparent for a more select national society. The local academies in the European capitals had become national institutions in a way that was not possible for the similar societies in the United States, owing to the lack of centralization. Our National Academy of Sciences was organized in 1863 with a membership at first limited to fifty, and still under one hundred. The Academy was intended to be the adviser of the Government in scientific matters, and has to a certain extent fulfilled this function. At first, when there were but few scientific men in the United States and their interests were more or less common, the National Academy was an organization fitted to its environment. But it has scarcely adjusted itself to the growth and specialization in science of the past twenty-five years.

The organization of science that was adequate for the third quarter of the century did not suffice for the fourth quarter. About twenty-five years ago it became necessary to meet the specialization becoming inevitable for scientific advance. Special societies and special journals were organized. The American Society of Naturalists, organized in 1883, and the 'American Naturalist,' established in 1867, covered a limited, but still wide field. 'Science,' a weekly journal, was established in 1883 to keep the sciences in touch with each other and men of science in touch with the general public. The American Chemical Society was organized in 1876, The American Ornithologists' Union in 1883, The Geological Society of America and the present American Mathematical Society in 1888, and there are now national societies for almost every science. Special journals were established during the same period—'The Bulletin of the Torrey Botanical Club' (1870), 'The Botanical Gazette' (1876), 'The American Journal of Mathematics' (1878), 'The American Chemical Journal' (1887), 'The American Journal of Morphology' (1887), 'The American Journal of Psychology' (1887), 'The American Geologist' (1888), 'The National Geographic Magazine' (1888), 'The American Anthropologist' (1888) and so on, in increasing numbers, to the present time. A similar movement toward specialization is evident in the development of elective courses in our colleges, of advanced work in our universities, and in many other directions.

The American Association for the Advancement of Science did not fail to adjust its organization to the growth and differentiation of science. In 1875 a formal division was made into two sections, one for the exact and one for the natural sciences, and in 1882 nine sections were established. At this time, when the Association had fitted itself to existing conditions, it enjoyed a most prosperous period in its his-

tory, the meetings being large and fruitful. Thus the attendance at Boston in 1880 was 997; at Montreal in 1882 it was 937, and at Philadelphia in 1884 it was 1,261. But with the organization and growth of the special societies and journals referred to above, the Association did not maintain its commanding position. The American Society of Naturalists, with a more compact membership, chose midwinter as its time of meeting, and other societies became affiliated with it. The special societies, consisting of groups of experts, appealed to the loyalty of their members more directly than did the larger and more amorphous Association. There was even lack of sympathy between these societies and the Association. The attendance at the meetings became smaller, and the total membership decreased. The more eminent men of science and the younger workers were not regularly in attendance at the meetings and were perhaps not even members of the Association. The programs of the sections became heterogeneous and sometimes did not reach a very high standard. The amateur and picnic elements were rather prominent, while at the same time they were mediocre. Many men of science regarded the Association as a survival that had outlived its usefulness.

But to-day no one acquainted with the most recent work of the Association will deny that it has entered on a new period of its history. This began with a change of attitude toward the special societies, replacing rivalry with cooperation. There was much opposition to the plan of letting the American Chemical Society meet in affiliation with the Association, but when this was accomplished chemistry at once became its strongest section. So it has been in other cases, where special societies have met in affiliation with the Association. At the recent New York meeting there were sixteen such societies including practically all national societies that hold summer meetings. Other improvements in the organization of the Association have been effected. The council has been strengthened and made a truly legislative and executive body. The permanent funds have been increased, and appropriations for research have been granted to committees. Care has been exercised in the election of fellows, and in the admission of titles to the programs. 'Science' is sent free of charge to all members, thus increasing and consolidating interest in the Association and in the advancement of science, giving even those unable to attend the annual meetings an adequate return for membership, and tending to unite all men of science and those interested in science in the Association and in the ends that it represents. The last three meetings, held at Boston, Columbus and New York, were all excellent, representing different types adjusted to the occasion and place. The meeting at Denver this year will be equally typical and equally successful. The membership of the Association is now larger

than it ever was before, over eight hundred new members having been elected within the past year.

There is every reason for satisfaction at the present condition and outlook of the Association. But this does not mean that we need not be on the alert to increase its usefulness under the circumstances confronting us at the beginning of the twentieth century. Evolution occurs by natural selection, but with boundless waste, regardless of time and careless of the individual. Human development must henceforth be guided by forethought and reason. It is the object of this article to make some definite suggestions regarding the organization of science in America under the auspices of the Association. They have been carefully considered by some of those most interested in the Association and, though they may not meet with universal approval, they are thought to be worth careful consideration.

The objects of the Association are said in its constitution to be "by periodical and migratory meetings, to promote intercourse between those who are cultivating science in different parts of America, to give a stronger and more general impulse and more systematic direction to scientific research, and to procure for the labors of scientific men, increased facilities and a wider usefulness." This statement may be somewhat systematized and amplified. The legitimate objects of the Association may be said to be (1) the presentation and discussion of research work in the different sciences and the publication of such research. (2) The promotion of research by grants of money and by providing the means for cooperation. (3) The encouragement of addresses, reports and publications on the progress of different departments of science, sometimes of value to the specialist, but more especially important in keeping the sciences in touch with each other. Joint meetings, discussions and publications should be arranged on subjects common to different sciences, relating the pure and applied sciences or concerned with science as a whole. (4) The presentation of such addresses, reports, discussions and publications in a form that will so far as possible keep the general public informed on the advances of science, interest them in the opportunities of scientific work and its needs, and impress on them the dignity and supreme importance of science. Here should be included whatever will secure recruits to scientific workers and the money and support that scientific work requires. (5) Offering an opportunity for men of science in different departments to become acquainted personally and by publication, and encouraging their sympathy and loyalty to their common interests and performing, so far as possible, the same function for scientific men and the intelligent public. (6) The guidance of scientific organization in America, which includes the coordination, establishment and arrangements for the meetings, etc., of special scientific societies; the

publication and circulation of scientific books and journals; the place of science in education and all external means for the advancement and diffusion of science; the direction of public opinion and legislation on science, more especially when connected with the national government, and the different states and municipalities; the promotion of conditions required by science and of reforms recommended by science —in general, whatever will promote the advancement, diffusion and usefulness of science.

1. The first of these functions has in large measure been assumed by the special societies and journals, and this is in accordance with necessary conditions. Special research must be presented before, and discussed by, small groups of experts and must be published in journals that are of interest only to specialists. The special societies have compact organizations; they are most competent to select their membership, to arrange their programs and to conduct their publications. It seems inevitable that the Association must relinquish its function of providing sections for the presentation of special papers, except in the rare case that a special society does not exist and may be formed by the aid of the Association. In a joint meeting of a special society and the corresponding section all the valuable papers will be presented both before the society and the section, and only such papers will be presented to the section alone as the society will not admit. There is, however, no reason why the present general organization should not be maintained, and the papers read before the affiliated societies be made part of the proceedings of the Association. The Association may, however, render important assistance to the special societies in the ways indicated below.

2. The promotion of research by grants of money and by providing the means for cooperation is a function that should be undertaken both by the special societies and by the general Association. The latter is, as a matter of fact, more likely to secure funds for this purpose by bequests and gifts, owing to its national character, its long history and its permanence. It can to special advantage further researches in which more than one science is concerned and in which independent societies might fail to cooperate. Efforts should be made to increase the number of patrons of the Association and to secure bequests and gifts, in order that the American Association may not be behind the British and French Associations, which appropriate annually $5,000 or more for the direct encouragement of research. Invested funds yielding an income for this purpose would add greatly to the stability, influence and usefulness of the Association, and to the interest of the meetings at which the grants are made and the reports of the work accomplished are presented.

3. The special societies may with advantage present addresses and

reports on the progress of a science, and, when the societies meet at the same time and place, their value is increased by the opportunity afforded for a larger group to be present. In this direction the Association has, however, an important work. The address of the president, the most eminent man of science in America who has not yet held this office, should be an event of national importance. It should be worth publication, and should be published in full in all the important daily newspapers, as actually happens in England in the case of the president of the British Association. The addresses of the vice-presidents should be as nearly as may be of the same importance and interest. These should not be addresses such as are presented before the special societies, but should be intelligible and interesting to all men of science and to the great mass of men and women who have had a college education or an equivalent training in affairs. The afternoons through the week might with advantage begin with these addresses, not more than two being given simultaneously, and these might be followed with reports or discussions of problems of general interest. The sectional committees and the council should pay special attention well in advance to the arrangement of a program. Care should be taken, if necessary, by invitation to those not members of the Association, to secure the adequate presentation of subjects in which the Association needs strengthening. Thus applied science should be given more prominence than hitherto. Those eminent in public life, in educational work and the like, and distinguished foreign men of science might be invited to address the Association or to take part in its discussions. Funds should be available to defray at least the traveling expenses of such invited guests.

4. The addresses, reports and discussions should, in part at least, be of such interest as to attract the general public, securing a large local attendance and being reported widely by the press. It is not possible, least of all in a democratic country, for science to isolate itself from common life. There must be special research that can be appreciated only by the expert, but as quickly as possible the progress of science should be made a part of the world's common stock of knowledge. The American Association should be one of the chief factors in the diffusion of science, and its annual meetings should be looked forward to by the general public as the occasion when for its benefit the year's progress in science and the contemporary state of science are exhibited in their outlines and in correct perspective. The meetings should typify the dignity and weight of science, so as to impress these on the minds of all. The sympathy and support of all the people are absolutely essential for science. Only so can recruits for scientific work be secured; only so can endowments and material support be obtained; only so can scientific work under the government be placed on a secure and permanent basis. We have in these needs not only the

justification, but the absolute necessity of an Association with a large membership—it should be at least ten thousand—drawn from the intelligent people of the whole country.

5. The social intercourse and personal contact of scientific societies and meetings are among their most important functions. Men in isolation become selfish and incompetent. Even a great genius does not work in solitude, and certainly the ordinary man requires the interest and enthusiasm that is only evoked in the give and take of personal acquaintance and conversation. Eating together, drinking together, smoking together, may have physiological drawbacks, but the psychological stimulus has warranted the origin and survival of the practices. Those studying similar problems, and those working in diverse directions; the university professor, the school teacher and the government officer; those who call their science pure and those who seek to make it useful; the beginners and the old benchers, all should be thrown together, ready to learn and help, to agree and differ. Each should be prepared to profit much, and if need be to sacrifice a little for the common good. The meetings of the Association do, of necessity, accomplish a great deal in bringing men together, but perhaps not all that could be desired. The cultivation of personal acquaintance between professional men of science and the amateur and outsider is also important, but more difficult to manage. The social features of the British Association seem to be more successful than our own. A thousand or more of the leading citizens of the place become temporary members for each meeting, and freely offer entertainments of one sort or another. The social conditions are, of course, different in America, but it seems that the entertainments and excursions might be made more pleasant and profitable in the future.

6. Of all the important functions of a national scientific association, the most essential is the general organization of science. The science of the country absolutely requires a central legislative body. Such bodies exist in other nations, having varying degrees of usefulness, and there is more need of an active and efficient representation of scientific interests in the United States than in any other country. London, Paris and the other European capitals, with their societies, clubs, etc., bring together all the scientific men of the country, whereas here they are widely scattered, and will become still more so as the East loses its intellectual precedence. Washington will doubtless be our chief center for scientific research, but under our system of State governments and with our privately endowed institutions, it is not likely that it will occupy the position of European capitals. The great development of scientific work under the national government, the numerous smaller centers under the State governments at their capitals and universities, the municipalities with their increasing

tendency to support museums, libraries, etc., and to undertake functions requiring scientific experts, the great incorporated universities developing special research, the applications of science in industries, transportation, etc.—all these represent an extraordinary activity, and, at the same time, a dispersion of tendencies and interests that require here more than in any country some unifying and centralizing organization. The functions of such a body are only limited by its efficiency. Our government recognizes a division into executive, legislative and judicial functions, but does not recognize the coordinate importance of expert opinion. As the judicatory interprets the laws made by the legislature, so the legislature requires impartial advice and scientific knowledge as the basis of its enactments.

The question now arises as to what body or bodies should perform the functions thus outlined. In the first place, it is evident that we need numerous and partly independent institutions. Each university, museum, survey, observatory, botanical garden, laboratory and the like is a unit, requiring its special organization. Each city should have a local academy, or alliance of societies, which in its field should perform most of the functions that we have been considering. Similar academies, or groups of societies, are needed for a State or region. National societies are required for each science. But what should be the national organization that will bring all the local and special societies together, and accomplish for the nation and for science as a whole what these institutions and societies do for a locality or a single science? We have at present the National Academy of Sciences and the American Association for the Advancement of Science, both of which have to a certain extent filled these requirements, but only in a partial and imperfect way. The Academy is legally the adviser of the government, the Association has brought into its organization a majority of the scientific men and many of the scientific societies of the country, but it seems probable that neither a small self-perpetuating body of eminent men nor a plebiscite of all scientific men will perform the duties required. Representative government, in spite of its partial failures, is the kind of government under which we should live and must live. We find this most nearly embodied in the council of the American Association. This council might be made the representative body for science in America.

If it be asked what the American Association and its council should do to assume the position assigned to them, the reply may fortunately be made: let them continue the work that they have already begun. The whole matter is one of attitude and spirit, rather than of constitution and by-laws. Let all scientific men be fellows of the Association, make the members representative of the intelligence of the country, unite all scientific societies and institutions in the organiza-

tion of the Association, make the meetings important and interesting, let the council assume and deserve authority.

While the position of the Association must depend chiefly on natural fitness and development and on the spirit and character of its members, there are certain changes in organization that deserve consideration. We shall suggest some modifications which appear to be either desirable at present or objects to be kept in view.

Affiliated societies should be represented on the council, and all scientific societies, whether national or local, should be affiliated with the Association. The number of representatives allowed from each society should be proportional to the number of members of the society among the fellows of the Association. For example, each institution having ten fellows might be allowed a representative and an additional representative for each additional twenty-five fellows. This plan includes the representation of local academies, universities, government departments, etc., on the council, but might begin with the societies meeting with the Association, in accordance with an amendment to the constitution now pending. It might be well for the council to elect each year three additional members to serve for a term of three years. Those so elected would probably be among the most efficient members of the council. The council would thus be considerably enlarged, but its authority would be greatly increased. It is of course understood that the real work of legislative bodies is done by committees, and the committees of the council should be organized with special care.

The executive officer of the Association is the permanent secretary, and his influence should be very great. He should either be paid a reasonable salary, say $5,000, and devote his whole time to the Association and the organization of science in America, or should be, as our present secretary, a man of unusual executive ability, having under him one or two assistant secretaries who should devote themselves to the work. The secretaries of the sections should be among the most efficient members of the sections, and should be elected for a term of three years and re-eligible.

The meetings should be more thoroughly organized in advance, more authority being vested in the permanent secretary and council. As suggested above, public lectures and discussions on the important advances and current problems of general interest should be arranged. For example, this year there should be reports on the relation of mosquitoes to disease, on the newly established Bureau of Standards, on the conduct of a national observatory, on the natural history and resources of the West Indies and the Philippines, and, in view of the place of meeting, on mining and irrigation.

The time of meeting has always interfered with success. Men of

science will not and cannot come together at midsummer. If a week can be set aside at the beginning of the year, it is probable that the scientific character and weight of the meetings will be greatly forwarded. The importance of obtaining a convocation week in midwinter has been emphasized in a recent editorial ('Science,' April 26, 1901), and we are now able to report that, of the fourteen universities composing the Association of American Universities, all but two either already have no exercises at the time or have altered their calendars in the direction of setting aside the week in which New Year's Day falls for the meetings of scientific and learned societies. It might, however, be well to have, say once in three years, a summer meeting in which the social and excursion elements should be emphasized. It must be remembered that the National Educational Association can bring together 10,000 members in this way. Or perhaps, it will be found with experience that the winter meeting is so advantageous that the summer meetings can be omitted altogether. Meanwhile there might be suggested a special meeting at Chicago next year at Christmas time in conjunction with the Naturalists and affiliated societies, the usual meeting at Pittsburg in midsummer, and a meeting of unusual importance at Washington at the end of the year.

SCIENTIFIC LITERATURE.

A HISTORY OF THE THERMOMETER.

DR. H. CARRINGTON BOLTON is one of the few Americans acquainted with the history of science, and his little volume on the evolution of the thermometer (The Chemical Publishing Company) represents a type of publication too rare in this country. The scientific information is correct throughout and is based on first hand knowledge, while at the same time the contents are sufficiently interesting to be read by any one. It is probably not generally known that the thermometer was invented by Galileo. When we remember that we owe to this one man not only the foundations of physical science, but also in large measure the pendulum, the compass, the telescope and the microscope, it may lead to a certain amount of modesty in our appreciation of modern inventions. Galileo, probably in 1595, invented the open air thermoscope; he determined the relative temperature at different places and at different seasons of the year and made experiments on freezing mixtures. In 1611 Sanctorius applied Galileo's instrument to the diagnosis of fevers. Ferdinand II. of Tuscany, to whom we owe the famous 'Accademia del Cimento,' first sealed the glass, making the instrument independent of atmospheric pressure. Many improvements were gradually made especially in the endeavor to find fixed points on a definite scale, the freezing point of water being first used by Robert Hooke in 1664. Of the three thermometers still in use, Fahrenheit's thermometer was invented in 1709, Réaumur's instrument in 1730 and the scale of Celsius in 1742. None of these thermometers, however, are now used in the form in which they were originally devised, and Dr. Bolton calls attention to the somewhat curious fact that the instrument constructed by the German, Fahrenheit, is used almost exclusively by English-speaking peoples; that invented by the Frenchman, Réaumur, is used chiefly in the north of Europe, while that of the Swede, Celsius, is used in the French-speaking countries. Dr. Bolton does not attempt to compare the usefulness of the three scales. The centigrade scale is, of course, the most logical, but, as sometimes happens in this world, it is not quite certain that it is the most convenient. When the scale of temperature between freezing and boiling water is divided into one hundred parts, the degrees seem to be somewhat too large for use in daily life, whereas if it were divided into one thousand parts they would be obviously too small. It is possible, however, that this is Anglo-Saxon prejudice, and that the centigrade degree measures temperature with sufficient accuracy for ordinary purposes, while its decimal subdivision must certainly be used hereafter for scientific work. Dr. Bolton's book is so small that it seems a pity that he did not add a chapter on the exact thermometric methods of the nineteenth century.

EXPERIMENTAL PSYCHOLOGY.

PROFESSOR E. B. TITCHENER, of Cornell University, has given us our first adequate laboratory manual of experimental psychology and has thus marked an epoch in the development of a science. Experiment in psychology, like much else, goes back to Aristotle, and has never since been entirely lacking. The great philosophers—Descartes, Hobbs, Kant and the rest—advanced

psychology as well as the other sciences. When the separate sciences developed, some part of psychology was taken with them, and the physicist, the physiologist and the zoologist made experiments and researches which are now claimed by psychology. In the meanwhile philosophy continued to care for psychology—we have, for example, in England Locke, Berkeley and Hume, and in Germany Herbart—but sometimes without sufficient attention to observation and experiment. Then about fifty years ago, in the hands of those who cared both for philosophy and science—Lotze, Fechner, Helmholtz, Wundt and others—psychology took definite shape as a natural and experimental science. Wundt's 'Physiologische Psychologie,' published in 1874, was the first comprehensive handbook. James's 'Principles of Psychology,' published in 1890, is equally important and more readable. Apart from numerous good text-books and treatises in various languages, we had the first laboratory manual in Sanford's 'Course in Experimental Psychology' (1894), but this only treated the senses which had already been pretty well worked over by physicists and physiologists. Now in Titchener's 'Experimental Psychology: a Manual of Laboratory Practice'—the work is published by the Macmillans—we have the first complete laboratory course in psychology. It is a large work: Volume I, which has alone been issued, includes two parts treating qualitative experiments, one intended for the student (xviii+214 pp.) and the other for the instructor (xxxiii+456 pp.). Two further volumes, treating quantitative experiments, are promised. The experiments are described in chapters entitled: Visual sensation, Auditory sensation, Cutaneous sensation, Gustatory sensation, Olfactory sensation, Organic sensation. The affective qualities, Attention and action, Visual space perception, Auditory perception, Tactual space perception, Ideational type and the association of ideas, Appendices.

Detailed comment and criticism must be relegated to the special journals. There is no question but that the work will greatly forward the teaching of experimental psychology and is invaluable to the teacher and advanced student. There will be difference of opinion as to how far the book can be put to advantage in the hands of students beginning laboratory work in psychology, and the question can only be settled by actual trial. There is naturally less agreement as to what experiments should be made and what methods should be used than in the case of sciences, such as chemistry and physics, where natural selection has long been at work. But Professor Titchener has laid the foundation on which future workers must build.

THE PROGRESS OF SCIENCE.

THE JOHNS HOPKINS UNIVERSITY AND PRESIDENT REMSEN.

THE election of Professor Ira Remsen to the presidency of the Johns Hopkins University has been received with general approval, and will be particularly gratifying to those who have been connected with the University as students or teachers and to men of science throughout the country. The Johns Hopkins University was incorporated in 1867; the founder died in 1873, and a year later Dr. D. C. Gilman was elected to the presidency. When the University opened its first session in 1876, Dr. Gilman had secured the services of a small but notable group of professors, of whom, since the lamented death of Rowland, but two remain—Professor Remsen and Professor Gildersleeve. President Gilman and his associates, freed somewhat from traditions and from the need of conducting a school for boys, erected at Baltimore a true university, distinctly in advance of any other American institution, except possibly Harvard, then just becoming subject to the influence of President Eliot—like Remsen, a professor of chemistry. In spite of the loss of a great part of its endowment, due not to carelessness on the part of the trustees but to the dictates of the founder, the Johns Hopkins University has maintained its position, and in the establishment of its medical school in 1893 has accomplished for medical education what had been accomplished earlier for university work. In the development of the university, Professor Remsen has always been President Gilman's chief associate and adviser, and is his natural successor. Remsen was graduated from the College of the City of New York in 1865 and received his M.D. from the College of Physicians and Surgeons, Columbia University, two years later. Studying abroad, he was made assistant in chemistry in Tübingen and was afterwards professor in Williams College, till his removal to the Johns Hopkins University in 1876. He has been given the LL.D. by Columbia and Princeton; is the foreign secretary of the National Academy of Sciences, and a member of many scientific societies. In his chemical laboratory and in 'The American Chemical Journal,' Professor Remsen has always upheld and forwarded the best ideals of research. As president of the Johns Hopkins University, he represents the highest type of educational leadership.

ACADEMIC FREEDOM HERE AND ABROAD.

QUESTIONS of academic freedom and the relations of university professors to authority are fully as troublesome abroad as in this country. It might be supposed that our system would work badly. The faculties have very little power, the authority being lodged in an absentee board of trustees and a president with almost absolute power; the trustees being usually and the presidents often men of affairs rather than scholars. The state universities are subject to political control, and the private universities are generally denominational and always dependent on the charity of patrons. Yet thanks to common sense and an appreciation of the importance of individual freedom, the university professor has in America a reasonably satisfactory status. There is little or no interference with the conduct of his department; his advance-

ment depends chiefly on efficiency rather than on favor; his position is permanent; he has complete freedom of research and reasonable freedom of speech and conduct. Those who are dissatisfied with the conditions here should make themselves familiar with what is happening abroad. We have recently had occasion to call attention to the troubles in the Royal Engineering College at Coopers Hill. Half the faculty was dismissed without a hearing by a board of visitors and a president, an army officer without academic experience. At this institution it appears that the professors are not even consulted as to the curriculum. An eminent chemist was dismissed from the University of Paris, because he believed that Dreyfus was not justly convicted; an eminent zoologist was compelled to leave the University of Zurich, because he took part in temperance reforms; now in Germany, supposed to be the home of academic freedom, we have events that could scarcely happen in America. The chair of zoology at Erlangen being vacant in 1897, the Bavarian 'Landtag' expressed the wish that the representatives of the natural sciences in the Bavarian universities should not be evolutionists. The associate professor of zoology at Erlangen, Dr. Albert Fleischmann, had published in 1896, the first part of a text-book of zoology based, like all recent works, on the theory of evolution. But when the second part was published in 1898, there was a remarkable conversion; a special chapter on the theory of evolution was added, declaring the theory to be absurd. The author was promoted to the professorship of zoology, and has now published a book, entitled: 'The Theory of Evolution: Popular Lectures on the rise and fall of a scientific hypothesis, delivered before students of all the faculties.' The book is not addressed to scientific men, but to the laity and clergy to whom the author owes his chair at the University of Erlangen.

SCIENTIFIC AND EDUCATIONAL ENDOWMENTS.

Two gifts of great importance to science have been made during the past month. Mr. Andrew Carnegie has created a fund of $10,000,000 for the Scottish Universities, and Mr. J. D. Rockefeller has established in New York an Institute for Medical Research. Mr. Rockefeller, in the endowment of the University of Chicago, has enjoyed the honorable distinction of having made the largest gift for public purposes, but even his great benefaction has now been surpassed by Mr. Carnegie.

The fund for the Scottish Universities has been transferred to trustees, in whose wisdom there will be perfect confidence, and no unwise restrictions have been placed on their power. At present, however, the income will be divided between paying the fees of students at the Scottish Universities, and strengthening the equipment and teaching staff. The scientific and medical departments, and modern languages and history, are designated as the subjects on which the money is to be spent. The Scottish Universities, like our own institutions, have always been close to the people, a very large percentage enjoying the benefits of a college training. An annual income of $500,000, devoted to higher education, will mean much for a comparatively small population.

Mr. Rockefeller's gift of $200,000 for an Institute for Medical Research is comparatively small. It is, however, intended for current expenses, and an endowment will doubtless be provided when required. The institute will be situated in New York City, but a building will not be erected at present, research being conducted in existing laboratories. The board of directors, with Dr. W. H. Welch of the Johns Hopkins University as president, guarantees the conduct of the institute in accordance with the highest scientific standards. It is most fortunate that we should now be on the way to share

with European countries the duties of organized medical research. There are institutions, such as Mr. Rockefeller has now founded, in Paris, Berlin, St. Petersburg and elsewhere, and last year Lord Iveagh established a similar laboratory in London. It is somewhat remarkable that great sums should have been spent annually on research in astronomy, geology and in other directions, whereas the advancement of medical science and its applications should have been left chiefly to individual effort. The first duty of the practising physician is to his patient, and the teacher in a medical school is doubly burdened, as he usually practises medicine, and at the same time instructs large classes. It is not surprising that the four hundred medical journals published in the United States are mostly somewhat dreary and barren, but, rather, that so much has been accomplished without state or private endowment.

THE CAUSES OF YELLOW FEVER AND OF CANCER.

WHAT can be accomplished by properly directed medical research is proved by two advances of extraordinary importance made recently by American students. These are the discovery of the probable causes of yellow fever and of cancer. In the present number of this journal, Surgeon-General Sternberg describes the experiments made under his direction by a board of army surgeons in Havana. In heroic self-sacrifice and triumphant achievement these experiments have surely an absorbing interest, surpassing any fiction. Although the yellow fever parasite has not been seen, its existence seems as certain as that of the malaria parasite. We now know that yellow fever is not directly contagious, but is transmitted by a special kind of mosquito, and, probably, only in this way. If we exterminate certain kinds of mosquitoes, or prevent them from biting those diseased, or from biting those who are well, two of the most dreadful diseases—yellow fever and malaria—will be exterminated. The cost in money and life of the Spanish-American War has been more than repaid to society by the services of the medical army officers.

Science is often said to be cosmopolitan, but men of science owe allegiance to their country, and there is every reason to rejoice that it is also to an American that we owe the discovery of the probable cause of cancer. Dr. Harvey R. Gaylord, working in New York State Pathological Laboratory at Buffalo, has been able to cultivate the organisms that cause cancer, and to produce cancer by injecting them into healthy animals. These organisms are not bacteria or yeast cells, but protozoa. There has long been a difference of opinion as to whether cancer is due to alterations in nutrition, or to a parasite. Now that the latter has been proved, cancer must be regarded as a preventable disease, and it remains to discover the method of its propagation. It must, of course, be remembered that Dr. Gaylord's discovery, like all others, rests on a long line of careful researches carried on in many countries. There are innumerable names connected with the development of the germ theory of disease, but the forerunners of Gaylord, who especially deserve mention in connection with cancer, are Scheuerlin, Kubasoff, Russell, Sanfelice and Plimmer.

THE BRITISH ANTARCTIC EXPEDITION.

THE resignation of Professor J. W. Gregory from the scientific staff of the British Antarctic Expedition is unfortunate, both because he possessed peculiar qualifications for his post, and because it has brought to light dissensions among those interested in the success of the expedition. The question at issue between the Royal Geographical Society, on the one hand, and the Royal Society, or some of its members, on the

other, is one frequently recurring, namely, should the executive command of scientific work be entrusted to a scientific man or is this unnecessary? When it was first arranged that Professor Gregory should take part in the expedition, it was understood that he would be the scientific leader. The British Government, however, gave a liberal subsidy, and a naval officer, Lieutenant Robert F. Scott, was appointed commander, Professor Gregory being made head of the civilian scientific staff. The relative position of Captain Scott and Professor Gregory gave rise to friction, Sir Clements Markham and the Royal Geographical Society holding that the scientific work was under the control of the naval officer in command. There were numerous conferences, and Professor Gregory finally consented to be satisfied with the control of a party to be landed on the coast. When, however, it was decided that the party should only be landed if this did not interfere with geographical exploration, Professor Gregory resigned. It seems evident that a scientific expedition can have but one leader, and it is natural that the Royal Geographical Society should regard exploration rather than geological and biological research as the primary object in the present case. The results will depend on the personality of Captain Scott, an unknown quantity in America; he may simply engage in adventure, or he may prove himself a competent scientific leader. The German expedition, with Dr. von Drygalski in absolute control, has, however, an advantage from the scientific point of view.

SCIENTIFIC NEWS.

PROFESSOR TRUMAN HENRY SAFFORD, who since 1877 had occupied the chair of astronomy at Williams College, died on June 13, at the age of 64 years.—Dr. Otto Lugger, State entomologist of Minnesota, and well known for his important contributions to economic entomology, died of pneumonia on May 21.—John Viriamu Jones, principal of University College, South Wales, and professor of physics in that institution, died on June 2, at the age of 45 years.—The eminent paleontologist, Professor Gustaf Lindström, keeper of the department of fossil animals in the Royal Museum, Stockholm, Sweden, died on May 16.

AT the annual meeting of the American Academy of Arts and Sciences, it was unanimously voted to award the Rumford Medal to Professor Elihu Thompson 'for his inventions in electric welding and lighting.'—One of the Carnegie Research Fellowships of the Iron and Steel Institute of Great Britain has been awarded to Mr. John A. Matthews, who at present holds the Columbia University Barnard Fellowship.

DR. FREDERICK PETERSON, of Columbia University, has been appointed by Governor Odell the medical member of the New York State Lunacy Commission. Dr. Peterson's appointment at the present time is especially fortunate, owing to the complications in connection with the State Pathological Institute, which will doubtless be settled with regard to the best interests of science and the care of the insane in the State hospitals.